THE

MATHEMATICAL GAZETTE

EDITED BY

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LONDON

G. BELL AND SONS, LTD., PORTUGAL STREET, KINGSWAY

VOL. XXXII JULY, 1948 No. 300

OUR THREE-HUNDREDTH NUMBER.

FOLLOWING the precedent set in No. 200 of the *Gazette*, opportunity has been taken to include in this special number portraits of some of those who have rendered valuable service to the Mathematical Association during the last twenty years. Notable omissions are explained by the fact that since 1934 the portraits of our Presidents have been given year by year in the *Gazette*.

Kenneth Scotchburn Snell was our Honorary Treasurer from 1936 to 1947; problems of finance during the war period, and the even more difficult problems of the immediate post-war years, made his work as Treasurer laborious and troublesome. For his skill and devotion the warm thanks of the Council were accorded to him on his retirement from the Treasurership, when he was elected a Vice-President.

George Lewingdon Parsons succeeded Charles Pendlebury as an Honorary Secretary in 1936. His untiring energy was a major factor in the task of keeping the Association in being during the difficult war and post-war years.

Mrs. E. M. Williams served the Association as an Honorary Secretary for the period 1939–1946. During the war most of the Association's activities were suspended, but the steady publication of the *Gazette* helped to maintain the Association and keep contact with members. This publication could not have been maintained had it not been for the constant and unwearying work of Snell, Parsons and Mrs. Williams.

Miss M. E. Bowman succeeded Mrs. Williams in 1946, and has followed the example set by Miss Punnett and fostered by Mrs. Williams in keeping the Association informed on the special problems of teaching mathematics to girls.

Alan Robson, Chairman of the Teaching Committee 1938–1946, has placed his wide experience of teaching and writing freely at the disposal of the Association, and our *Reports* owe a great deal to his tireless industry.

Cyril Tetlow Daltry, to whom the London Branch is deeply indebted for many years of service as Secretary, was Secretary of the Teaching Committee for the period 1938–1947, and has contributed largely to the preparation of that Committee's *Reports*.

Walter James Langford succeeded Robson as Chairman of the Teaching Committee in 1946. A large programme of *Report* production, necessarily suspended during the war, has now been resumed under his energetic guidance.

Albert James Gooding May, Secretary of the Branches Committee, has played a large part in coordinating the revival of Branch activities, so marked a feature of the post-war work of the Association.

Francis William Kellaway, as Secretary of the Programme Committee, has organised the very successful recent Annual Meetings of the Association; he has also most generously assisted the Editor in the work of producing the *Gazette*.

Thomas Arthur Alan Broadbent has edited the *Gazette* since January 1931 (Nos. 211–300).

In addition to these portraits, there are two other plates. As frontispiece, we reproduce the cover of the first number of the *Gazette* (April, 1894). We are also glad to be able to give a photograph of part of the case-room of the Glasgow University Press. From No. 7 onwards the *Gazette* has been printed at Glasgow by Messrs. Robert MacLehose & Co.; the typographical skill given to this work is evident on every page of the *Gazette*, but perhaps only the Editor can fully appreciate the patience and enthusiasm with which Messrs. MacLehose have met the Association's requirements.

In pp. 99–159 the reader will find reprinted a few of the outstanding articles and reviews which have appeared in earlier *Gazettes*; the "Gleanings" in this number are also all reprints of earlier items. The remainder consists of new material, much of it specially written for this number. The Editor wishes to thank all those who have so generously responded to his requests. One promised article was never written; Sir D'Arcy Wentworth Thompson's death prevented him from carrying out the plan suggested in his letter, from which we quote: "I am pleased indeed—my vanity is tickled—by your invitation to contribute to your 300th number. The *Gazette* is *sui generis*, and I hold it in regard and even affection. . . . What would you say to a short article on Euclid . . . the small number of important propositions, the real στοιχεῖα: like landings on a staircase, while all the rest are the little steps up to them, very boring to schoolboys like Clerk Maxwell or P. G. Tait, but all of them necessary in the logical ascent from the first of regular plane figures to the last of regular solids."

G. H. HARDY.

THE kindness of Professor Borel enables us to add to the obituary notice of Professor Hardy a fact which shows the esteem in which Hardy was held in France. Professor Borel writes:

"G. H. Hardy avait été élu, quelques mois avant sa mort, membre associé étranger de l'Académie des Sciences de Paris. Le nombre de ces membres associés ne peut dépasser *dix*, pour l'ensemble des nations et pour toutes les disciplines scientifiques. Il y a donc seulement un ou deux mathématiciens qui reçoivent cette distinction dans le monde entier."

THE ECCENTRIC CIRCLE OF BOSCOVICH.

By E. M. Langley.

[*Mathematical Gazette*, No. 1 (April 1894)]

The object of the following paper is to draw attention to a general and powerful but quite elementary method of transformation, by means of which the properties of a *conic* may be inferred from those of a *circle*. It has many striking analogies with the method of perspective transformation, and a simple geometrical connection with it. It is, however, of much simpler character for constructive purposes, and though it is intended to show later on how well the method is adapted to serve as an introduction to modern methods, it is hoped that readers will begin by dismissing from their minds all notions of *cross ratios*, *homographic ranges*, etc., and regard the theorems presented to them from the point of view of a student who has mastered his *Elements of Plane Geometry*.

I. A sketch of Boscovich's own treatment of the conic by means of the eccentric circle.

1. Having defined a conic as a plane curve by means of its *focus* and *directrix*,* he proceeds (p. 39) to solve in the most general manner the following problem: *Having given the focus, directrix, and eccentricity of a conic, to find its intersection with a given straight line* (Fig. 1–5).

Let F be the focus, AB the directrix, and e the eccentricity of the conic; HK the given straight line, cutting AB in H. Take *any* point L on either side of the directrix AB and draw LG perpendicular to AB. Along LG take LS equal to eLG. With centre L and radius LS describe a circle STt. Join FH. Through L draw a parallel LO to HK, meeting AB in O. Through O draw OZ parallel to FH, cutting the circle STt in Tt. Join LT, Lt. Through F draw FP, Fp parallel to LT, Lt, cutting HK in P, p.

Then P, p are the points required.

Draw PD perpendicular to AB. By similar triangles FPH, LTO

$$FP : PH :: LT : LO.$$

By similar triangles HPD, OLG

$$PH : PD :: LO : LG,$$

$$\therefore\ FP : PD :: LT : LG.$$

But
$$LT = eLG,$$

$$\therefore\ FP = ePD.$$

$\therefore$ P lies on the conic. Similarly p lies on the conic.

Conversely, if HK meet the conic in P, and FH, LO, OT be drawn as before, if LT be drawn parallel to FP, it can be proved that

$$LT : LG :: FP : PD.$$

$$\therefore\ LT = eLG.$$

$\therefore$ T lies on the circle.

* According to Dr. Taylor, Boscovich was the first to write a really complete elementary treatise on conic sections based on the eccentricity or "determining ratio". See *Ancient and Modern Geometry of Conics*, p. lxxii, where the work of Boscovich is commended as "a clear and compact treatise, which for simplicity, depth and suggestiveness, will not readily be surpassed". The references to Boscovich's own treatise (*Elementorum Universae Matheseos*, tom. iii) are always to the Venice edition, 1757.

$\therefore$ HK cannot meet the conic in any points but those found by the above construction. We shall follow Dr. Taylor in calling the circle with centre L and radius eLG *the eccentric circle of the point* L.

After remarking in a scholium: "Mirum sane quam foecunda est haec constructio, quam Tyroni exercendo apta", he points how the construction may be simplified by taking the point L in certain special positions.

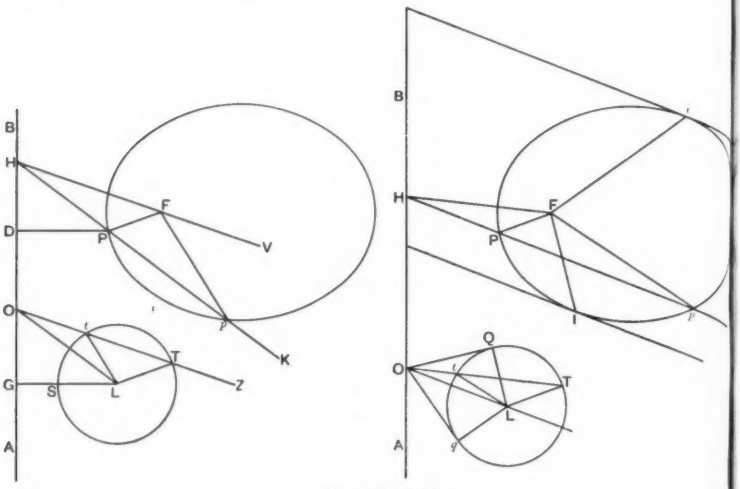
(i) *On the conic*: in which case the radius of the circle is obviously the focal distance of its centre, and the circle therefore passes through the focus.

(ii) *At the focus*: in which case the radius is equal to the semi-latus rectum. *This is an important case; it will be treated specially in discussing the connection between Boscovich's transformation and that obtained by conical projection.*

(iii) *At the centre of the conic*: in which case the circle becomes the auxiliary circle of the conic.

(iv) *On the given straight line whose points of intersection with the conic are sought.* This case also is important. The construction obtained in it is that on which Haslam and Edwards base their *Conic Sections treated Geometrically* (Longmans, 1881). It will be treated specially in the next section.

2. He then points out that all the points on a conic can be found from the general construction by moving the line HK parallel to itself, the point H traversing the directrix AB. For if HK keeps its direction, the line LO and therefore the point O remains fixed, while the line OZ turns round the fixed point O.

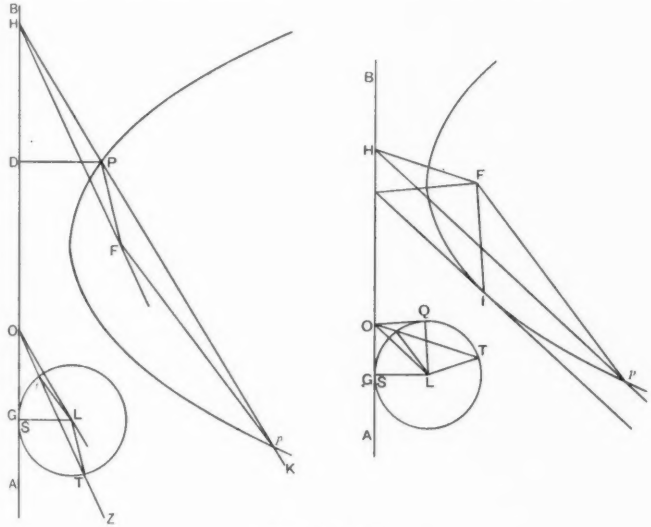


FIGS. 1 and 6.

Taking first the case of the ellipse (Figs. 1, 6) in which, e being less than unity, the eccentric circle lies entirely on the same side of AB as L is. Let H start from a distant point along OA and move to a distant point along OB.

Then OZ will turn from a position as near as we please to OB to one as near as we please to OA. It will at first not cut the circle ; then it will touch it at Q ; then it will continue to cut it at two points T, t, until it finally touches it again at q, after which it will not meet it. Hence as the line HK moves parallel to itself it will at first not meet the ellipse ; then it will touch it at a point I corresponding to Q ; then it will continue to cut the ellipse in points P, p corresponding to T, t, until it finally touches the ellipse again at a point i corresponding to q, after which it will not meet it.

Hence of all straight lines parallel to a given straight line two touch a given ellipse each in one point ; of the rest all which lie between these two cut it each in two points, while all those which lie beyond these do not meet it at all.



FIGS. 2 and 7.

Proceeding to the parabola (Figs. 2, 7), in which, e being equal to unity, the eccentric circle touches the directrix AB in G. Let H traverse the directrix as before, HK making a constant acute angle AHK with AB. As in the case of the ellipse, OZ, turning about O, will at first not cut the circle ; then it will touch it at a point Q ; then it will continue to cut it in two points T, t, however near OZ moves to OA. Hence as the line HK moves parallel to itself it will at first not meet the parabola ; then it will touch it at a point I corresponding to Q ; then it will continue to cut it in two points P, p corresponding to T, t, however far H be taken along AB.

Hence of all straight lines parallel to a given straight line which is not perpendicular to the directrix, one touches the parabola in a single point ; of the rest each either cuts it in two points or does not meet it at all, according as it lies on the same side of the tangent as the focus is or on the opposite side.

Next let HK be perpendicular to AB. Then O coincides with S and G ; the line OZ always cuts the circle in two points T and O (t always coinciding

with O) (Fig. 8). Hence as the straight line HK moves it will always cut the parabola in a single point P corresponding to T, the other point p corresponding to t receding to an infinite distance.

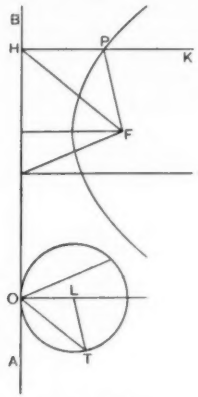


FIG. 8.

*Hence of all straight lines perpendicular to the directrix of a parabola each cuts it in a single point only, the other point receding to an infinite distance, so that it disappears (alterâ intersectione ita in infinitum abeunte ut nusquam jam sit).**

In dealing with the hyperbola he considers separately the three cases in which the angle AHK, remaining constant as before, is less than, equal to, or greater than the angle LNn (Figs. 3, 4, 5) which he calls the "angle of equality".

In the first case *two of the parallels touch the curve, one in each branch, the others either cutting one branch in two points or not meeting the curve at all.*

In the second, which is important as leading to the consideration of the asymptotes, *one only of the parallels fails to meet the curve, although it approaches each branch by less than any given distance, however small; of the rest each meets the curve only in a single point, the other point of intersection receding to infinity "ut nusquam jam sit".*

In the third case *all the parallels cut the curve in two points, one on each branch.*

3. The following propositions, which might with advantage have preceded the discussion on the sets of parallels, are then shown to follow from the general construction as simple corollaries.

A straight line cannot cut a conic section in more than two points, or touch it in more than one.

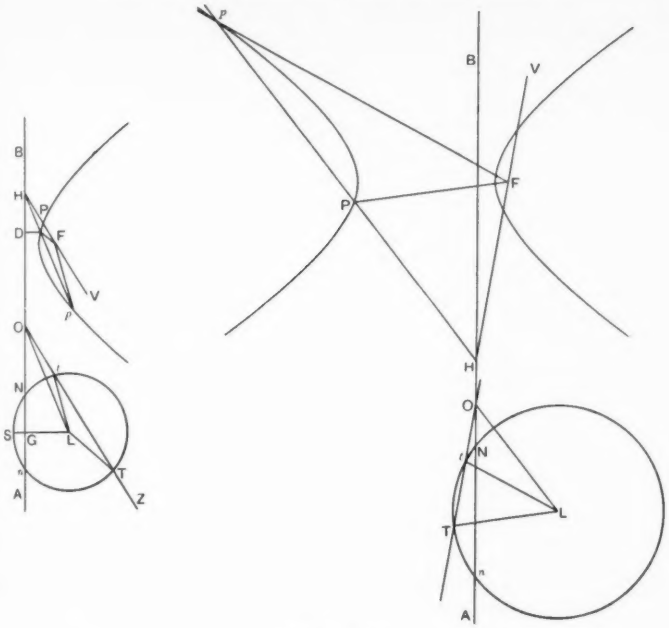
For OZ cannot cut the *circle* in more than two points, or touch it in more than one.

The line joining the focus to the point where a secant cuts the directrix is equally inclined to the lines joining the focus to the points where it cuts the directrix.

For since OZ is equally inclined to LT, Lt, it follows that FH is equally inclined to FP, Fp. The limiting case is noted.

4. An ingenious proof involving chord properties only is given of the proposition that the locus of the mid-points of a set of parallel chords is a straight

* This expression *ut nusquam jam sit* (or *sint*) is of very frequent occurrence in Boscovich.



FIGS. 3 and 4.

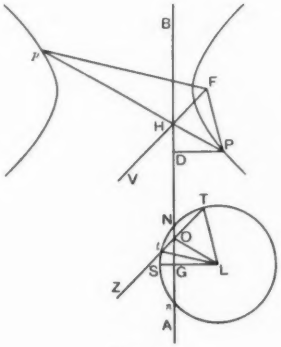


FIG. 5.

line. We supply the demonstration and figure for the case of the ellipse. Slight modifications only are required for the parabola and hyperbola.

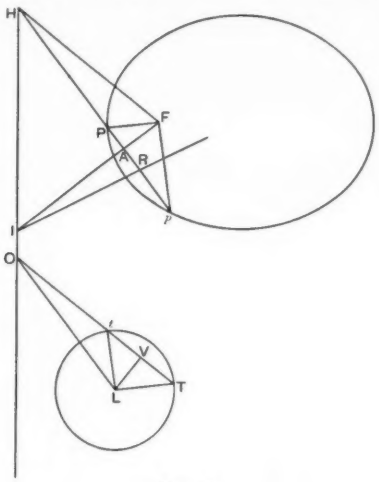


FIG. 9.

With the same construction as in Fig. 1 draw FA perpendicular to HPp, meeting the directrix in I. Bisect Pp, Tt in R, V. Join LV, RI.

By similar triangles

$$HP : HF :: OL : OT$$
$$HF : Hp :: Ot : OL$$
$$\therefore\ HP : Hp :: Ot : OT$$
$$\therefore\ HP + Hp : HP :: Ot + OT : Ot.$$
$$\therefore\ HR : HP :: OV : Ot.$$

But
$$HP : HF :: OL : OT,$$

and
$$HF : HA :: OL : OV.$$
$$\therefore\ HR : HA :: OL^2 : Ot \,.\, OT.$$

But $OT \,.\, Ot$ is constant. $\therefore$ R lies on a straight line through I.

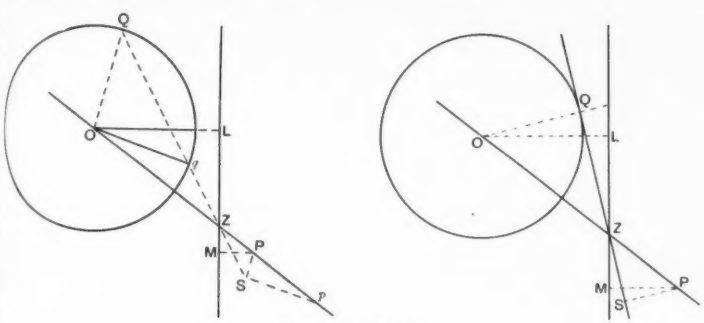
5. The special case of the general construction in which the centre L of the eccentric circle lies on the straight line whose points of intersection with the conic are required, is specially discussed and utilised, amongst other things, for drawing tangents from an external point, and for proving the ratio of the rectangles under the segments of two chords through a point, independent of its position, so long as their directions remain unchanged, but as the next section is devoted to this very case no details of his proofs are here given.

[*Mathematical Gazette*, No. 3, December 1894]

II. Consideration of the special case in which the centre of the eccentric circle lies on the straight line whose points of intersection with the conic are required.

In what follows it will be convenient to abandon the system of lettering adopted

by Boscovich which we have hitherto followed, and as far as possible to use that common to modern textbooks.



FIGS. 10 and 11.

1. *Having given the focus, directrix, and eccentricity of a conic, to find its intersection with a given straight line.*

Let the given straight line HK (Fig. 10) cut the directrix in Z. Take any point O on HK and draw OL perpendicular to the directrix. With centre O and radius $e \,.\, OL$, describe a circle cutting SZ in Q, q. Through S draw SP, Sp parallel to OQ, Oq meeting HK in P, p. Then P, p are the required points of intersection.

Draw PM perpendicular to the directrix.

Then $SP : PZ :: OQ : OZ$
and $PZ : PM :: OZ : OL.$ } by similar triangles.

$\therefore\ SP : PM :: OQ : OL.$

$\therefore\ SP = e \,.\, PM.$

$\therefore$ P lies on the conic. Similarly, p lies on the conic.

Conversely, it may be shown that if a straight line HK, meeting the directrix in Z, cut the conic in P, p and from a point O on HK straight lines OQ, Oq are drawn parallel to SP, Sp to meet SZ in Q, q,

$$OQ = e \,.\, OL = Oq,$$

when OL is perpendicular to the directrix.

Hence (i) SZ is equally inclined to SP, Sp; (ii) HK cannot cut the conic in more than two points.

2. *To find any number of points on a conic whose focus, directrix, and eccentricity are given.*

From any point O (Fig. 10) draw OL perpendicular to the directrix. With centre O and radius $e \,.\, OL$ describe a circle. Draw any radius OQ. Join SQ, cutting the directrix in Z. Draw SP parallel to OQ, cutting OZ in P. Then, by the above demonstration, P is a point on the conic.

It is worthy of remark that

(i) $SP : OQ :: SZ : ZQ$.

(ii) The ratio $SZ : ZQ$ is finite except where Q coincides with Z.

Hence when

$e < 1$, SP always remains finite.

$e = 1$, SP becomes indefinitely great only as its direction approaches that of OZ.

$e > 1$, SP becomes indefinitely great when its direction approaches that of either ON, On, N, n being the points where the circle cuts the directrix.

Returning to proposition 1, it is clear (i) that if Q, q coincide so also do P, p, and conversely. Hence, if SQ touches the circle, OP touches the conic, and conversely. (ii) If the tangent at P meet the directrix in Z then ZSP is a right angle. Hence the solution of the following problem :

3. *From a given point to draw a pair of tangents to a conic whose focus, directrix, and eccentricity are given.*

From the given point O (Fig. 11) draw OL perpendicular to the directrix. With centre O and radius $e \,.\, OL$ describe a circle, and from S draw SQ touching the circle in Q and cutting the directrix in Z. Then OZ shall be a tangent to the conic. Draw SP parallel to OQ, meeting OZ in P, and draw PM perpendicular to the directrix.

Then $$SP : PZ :: OQ : OZ.$$

And $$PZ : PM :: OZ : OL.$$

$$\therefore\ SP : PM :: OQ : OL.$$

$$\therefore\ SP = e \,.\, PM.$$

$\therefore$ P lies on the conic. And since angle ZSP = angle OQZ, which is right, therefore OP is a tangent. Similarly, a second tangent can be drawn.

Conversely, if from a point O on the tangent at P to a conic which meets the directrix in Z, perpendiculars OQ, OL be drawn to SZ and the directrix, then $OQ = e \,.\, OL$.

It easily follows that if OP, Op are tangents to a conic from O, then angle OSP = angle OSp. (See Haslam and Edwards, p. 9.)

III. General treatment of Boscovich's method as one of transformation.*

1. Def. *Let AB* (Fig. 12) *be a given fixed straight line, S and O two given fixed points, SP and OQ any pair of parallels through S and O, then if OP, SQ intersect on AB, the points P and Q are said to "correspond" to each other.*

Note that if Z be the point of intersection of OP, SQ,

$$SP : OQ :: SZ : QZ.$$

Hence, if Q approaches indefinitely near to AB, P recedes to infinity. Similarly if P approaches indefinitely near to AB, Q recedes to infinity.

From this property, the line AB will be called *the vanishing line* and be denoted by v.l.

2. Prop. *If P describes a straight line PH meeting the vanishing line AB in H, then Q describes a straight line LQ parallel to SH.*

Let the parallel to PH (Fig. 12) through O meet AB in L. Then L is a fixed point. Join LQ.

By similar triangles $$LZ : ZO :: HZ : ZP.$$

And $$ZO : ZQ :: ZP : ZS.$$

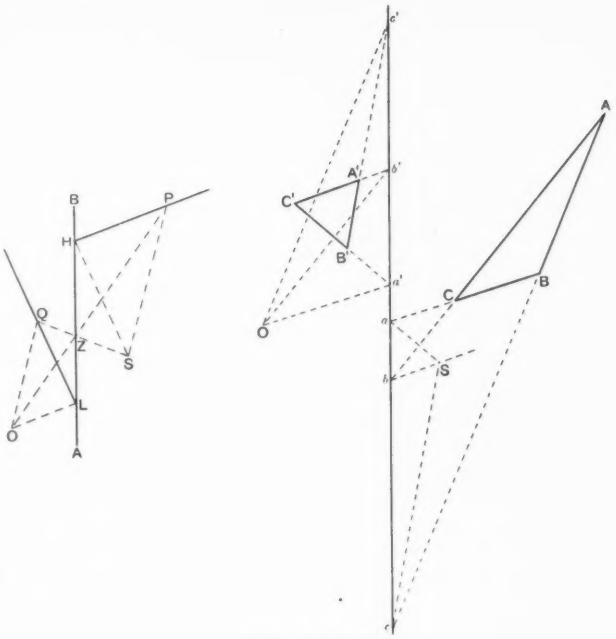
$$\therefore\ LZ : ZQ :: HZ : ZS.$$

And $$\text{angle } LZQ = \text{angle } HZS.$$

$$\therefore\ \text{angle } ZLQ = \text{angle } ZHS.$$

* In a paper on *The Discovery and the Geometrical Treatment of the Conic Sections*, read before the A.I.G.T. in 1884 and published in the Tenth General Report, Dr. Taylor strongly advocated the use of Boscovich's eccentric circle, and remarked on it as "one of the simplest introductions to homographic transformation in general".

$\therefore$ Q lies on a fixed straight line LQ parallel to SH. Hence the following definition :



FIGS. 12 and 13.

3. *Corresponding straight lines are such that each is the locus of a point which corresponds to a point on the other.*

Instead of proceeding in the above order it is plain that

(i) We might have first given the following definition of corresponding straight lines :

Let AB be a given fixed straight line, O and S any two given fixed points, HP, LQ any two lines drawn from points H and L on AB parallel to OL, SH ; then the lines HP, LQ are said to " correspond " to each other.

(ii) We might then have proved the following proposition :

If a straight line PH pass through a fixed point P, then the corresponding straight line LQ passes through a fixed point Q such that SP and OQ are parallel.

(iii) We might then have defined *corresponding points* as *the points of intersection of corresponding straight lines.*

The process of obtaining from a given straight line PH the straight line LQ which corresponds to it is called *reversion.* To distinguish between the original PH and LQ the derived line LQ is called the *reverse* of PH, while PH is called the *obverse* of LQ.* Note that the relationship is not strictly reciprocal unless O and S coincide.

* See Dr. Taylor's *Ancient and Modern Geometry of Conics.*

4. *A given angle may be reverted into an angle of any given magnitude.*

Let the arms CB, CA (Fig. 13) of the given angle BCA meet the v.l. in a, b, and let S be any point on the arc aSb containing an angle of the given magnitude. Through O draw parallels to aC, bC meeting the v.l. in a', b'. Through a', b' draw $a'C$, $b'C$ parallel to Sa, Sb. Then angle $a'Cb'$, which is the reverse of angle aCb, has the given magnitude.

5. *A given triangle may be reverted into an equilateral triangle.*

Let the sides BC, CA, AB of the given triangle meet the v.l. in a, b, c (Fig. 13). Let S be the intersection of the circumcircles of the equilateral triangles described on ab, bc. Through O draw parallels to BC, CA, AB meeting the v.l. in a', b', c'. Through a', b', c' draw parallels $B'C'$, $C'A'$, $A'B'$ to Sa, Sb, Sc. Then triangle $A'B'C'$, which is the reverse of triangle ABC, is equilateral.

6. *Any quadrilateral may be reverted into* (i) *a parallelogram ;* (ii) *a rectangle ;* (iii) *a square.*

(i) Let AB, CD meet in E ; BC, DA in F ; then for all positions of S the reverse quadrilateral is a parallelogram, if we revert EF to infinity.

(ii) If S is taken anywhere on the circle which has EF for a diameter, the reverse quadrilateral will be a right-angled parallelogram.

(iii) Let the diagonals AC, DB meet EF in H, K. Then if S be taken at one of the points of intersection of the circles whose diameters are EF, HK, the diagonals as well as the adjacent sides of the reverse quadrilateral will be at right angles. Hence it is a square.

7. *Tangents revert into tangents.*

Hence all properties of poles and polars revert.

8. *The cross ratio of a pencil is unaltered by reversion.*

Let the rays of a pencil whose vertex is P meet the v.l. in a, b, c, d, and the corresponding rays through the reverse point Q meet the v.l. in a', b', c', d'.

Then
$$Q(a'b'c'd') = S(abcd)$$
$$= P(abcd).$$

9. *Hence the anharmonic property of a conic.* For by taking S at the focus and the directrix for v.l. a conic can be reverted into a circle. Hence also *a conic reverts into a conic.*

10. If the polar of S be taken for vanishing line a conic reverts into a conic whose centre is O.

11. *A given conic reverts into* (iii) *an ellipse*, (ii) *a parabola*, (i) *a hyperbola, according as the v.l.* (i) *cuts*, (ii) *touches*, (iii) *does not meet the given conic.*

12. *If a chord of a conic subtend a right angle at a fixed point S on the conic, it always passes through a fixed point on the normal at S (Frégier's Theorem).*

The following neat proof is given by Haslam and Edwards, p. 101 :

Let Pp, $P'p'$ (Fig. 14), two of the chords subtending right angles at S, intersect in E. Take the polar of E for v.l.

Then the corresponding chords Qq, $Q'q'$ of the reverse conic intersect at F, which must be its centre since its polar is at infinity.

But QOq, $Q'Oq'$ are right angles.

Hence
$$QF = OF = qF = Q'F = q'F.$$

$\therefore$ The reverse conic is a circle. Hence the reverse $Q''q''$ of any other chord $P''p''$ subtending a right angle at S must pass through F, since $Q''Oq''$ is a right angle. Hence $P''p''$ must pass through E.

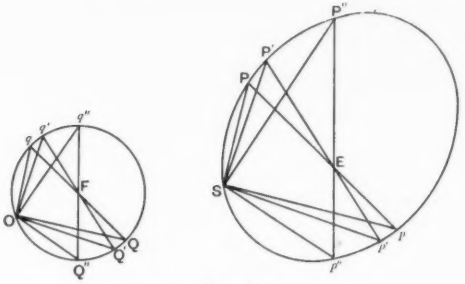


FIG. 14.

13. *Any conic can be reverted into a circle, any straight line which does not cut it being taken for the vanishing line.*

For the proof, see Haslam and Edwards, p. 99.

IV. Connection between reversion and perspective projection.

It requires very little acquaintance on the part of the student with the method of perspective projection to enable him to discover the close analogy of the results obtained by the method of *reversion* and by that of *perspective projection*, the resemblance extending even to minute details of construction. Geometrical instinct naturally leads us to suppose that there must exist some close relationship between the two methods. We proceed to demonstrate its existence.

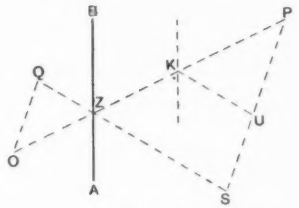


FIG. 15.

On the opposite side of the vanishing line AB to O (Fig. 15) and at an equal distance from it draw a straight line parallel to it. Let OP cut this parallel in K, and draw KU parallel to SQ to meet SP in U.

Then
$$SU : SP :: ZK : ZP$$
$$:: OZ : ZP$$
$$:: OQ : SP.$$
$$\therefore\ SU = OQ.$$

$\therefore$ The locus of U is congruent with that of Q. But U is the perspective projection of P, S being the *vertex* and HK the *axis of perspective*.

Now there is no reason why O and S should not coincide, and then Boscovich's transformation will be absolutely identical with the perspective projection. All we have to do is to take our axis of perspective on the opposite side of the vanishing line to the vertex S and at an equal distance from it. The figure is worthy of some attention. E. M. L.

THE AIMS OF EDUCATION—A PLEA FOR REFORM.*

By A. N. Whitehead.

When I had the honour of being made President of the Mathematical Association, I did not foresee the unusual responsibility which it entailed. It was my intention to take as the theme of a presidential address the consideration of some aspect of those special subjects to which my own researches have principally been directed. Events have forced me to abandon that intention. It is useless to discuss abstract questions in the midst of dominant practical preoccupation. We cannot disregard the present crisis in European civilisation. It affects every function of life. In the harder struggle for existence which lies before the nation, all departments of national effort will be reviewed for judgment. The mere necessity for economy in resources will provoke this reformation.

We are concerned with education. This Association, so rich in its membership of educationalists, with the conception of reform as the very reason of its being, is among those bodies which must take the lead in guiding that educational reconstruction which by a sociological law follows every social revolution. We do not want impracticable ideals, only to be realised beyond the clouds in

"Some wild, weird clime,
Out of Space, and out of Time."

We require to know what is possible now in England, a nation conscious of its high achievements, and of great failures, shaken to its foundations, distrustful of the old ways, and dreading fantastic novelties.

I will take my courage in both hands, and put before you an outline of educational principles. What I am going to say is of course entirely without your authority, and does not pledge or prejudge any action of the Association. We are primarily concerned only with the intellectual side of education, and, as mathematicians, are naturally concerned to illustrate details more particularly by reference to mathematics. Thus much to explain deliberate omissions in what follows.

Consider now the general and special education of two types of boys, namely those in secondary schools who in after life must form the professional and directing classes in commerce, industry, and public administration, and again those in junior technical schools, and later in advanced continuation classes, who are going to form the class of skilled artisans and foremen of workshops. These two sets compose the educated strength of the nation. We must form no ideals which include less than these entire classes within their scope. What I shall say will in phraseology apply more directly to the secondary schools, but with unessential changes it will apply equally to the other group.

What is the first commandment to be obeyed in any educational theme? It is this: Do not teach too many subjects. The second command is this: What you teach, teach thoroughly. The devil in the scholastic world has assumed the form of a general education consisting of scraps of a large number of disconnected subjects; and, with the artfulness of the serpent, he has entrenched himself behind the matriculation examination of the University of London, with a wire entanglement formed by the Oxford and Cambridge schools' examination.

Culture is activity of thought, and receptiveness to beauty, and humane feeling. Scraps of information have nothing to do with it. A merely well-informed man is the most useless bore on God's earth. What we should aim at producing is men who possess both culture and expert knowledge in some

* Presidential Address to the Mathematical Association, 1916. Printed in the *Mathematical Gazette*, January 1916.

special direction. Their expert knowledge will give them the ground to start from, and their culture will lead them as deep as philosophy and as high as art. We have to remember that the valuable intellectual development is self-development, and that it mostly takes place between the ages of 16 and 30. As to training, the most important part is given by mothers before the age of 12. A saying due to Archbishop Temple illustrates my meaning. Surprise was expressed at the success in after-life of a man, who as a boy at Rugby had been somewhat undistinguished. He answered, " It is not what they are at eighteen, it is what they become afterwards that matters."

In training a child to activity of thought, above all things we must beware of what I will call " inert ideas "—that is to say, ideas that are merely received into the mind without being utilised, or tested, or thrown into fresh combinations.

In the history of education, the most striking phenomenon is that schools of learning, which at one epoch are alive with a ferment of genius, in a succeeding generation exhibit merely pedantry and routine. The reason is, that they are overladen with inert ideas. Education with inert ideas is not only useless : it is, above all things, harmful—*Corruptio optimi, pessima.* Except at rare intervals of intellectual ferment, education in the past has been radically infected with inert ideas. That is the reason why uneducated clever women, who have seen much of the world, are in middle life so much the most cultured part of the community. They have been saved from this horrible burden of inert ideas. Every intellectual revolution which has ever stirred humanity into greatness has been a passionate protest against inert ideas. Then, alas, with pathetic ignorance of human psychology, it has proceeded by some educational scheme to bind humanity afresh with inert ideas of its own fashioning.

Let us now ask how in our system of education we are to guard against this mental dry rot. We recur to our two educational commandments, " Do not teach too many subjects," and again, " What you teach, teach thoroughly."

The result of teaching small parts of a large number of subjects is the passive reception of disconnected ideas, not illumined with any spark of vitality. Let the main ideas which are introduced into a child's education be few and important, and let them be thrown into every combination possible. The child should make them his own, and should understand their application here and now in the circumstances of his actual life. From the very beginning of his education, the child should experience the joy of discovery. The discovery which he has to make, is that general ideas give an understanding of that stream of events which pours through his life, which is his life. By understanding I mean more than a mere logical analysis, though that is included. I mean " understanding " in the sense in which it is used in the French proverb, " To understand all, is to forgive all ". Pedants sneer at an education which is useful. But if education is not useful, what is it? Is it a talent, to be hidden away in a napkin? Of course, education should be useful, whatever your aim in life. It was useful to Saint Augustine, and it was useful to Napoleon. It is useful, because understanding is useful.

I pass lightly over that understanding which should be given by the literary side of education. It is not peculiarly the function of this Association to consider it. Nor do I wish to be supposed to pronounce on the relative merits of a classical or a modern curriculum. I would only remark that the understanding which we want is an understanding of an insistent present. The only use of a knowledge of the past is to equip us for the present. No more deadly harm can be done to young minds than by depreciation of the present. The present contains all that there is. It is holy ground ; for it is the past, and it is the future. At the same time it must be observed that an age is no less past if it

existed two hundred years ago than if it existed two thousand years ago. Do not be deceived by the pedantry of dates. The ages of Shakespeare and of Molière are no less past than are the ages of Sophocles and of Vergil. The communion of saints is a great and inspiring assemblage, but it has only one possible hall of meeting, and that is, the present ; and the mere lapse of time through which any particular group of saints must travel to reach that meeting-place, makes very little difference.

Passing now to the scientific and logical side of education, we remember that here also ideas which are not utilised are positively harmful. By utilising an idea, I mean relating it to that stream, compounded of sense perceptions, feelings, hopes, desires, and of mental activities relating thought to thought, which forms our life. I can imagine a set of beings which might fortify their souls by passively reviewing disconnected ideas. Humanity is not built that way—except perhaps some editors of newspapers.

In scientific training, the first thing to do with an idea is to prove it. But allow me for one moment to extend the meaning of " prove " ; I mean—to prove its worth. Now an idea is not worth much unless the propositions in which it is embodied are true. Accordingly an essential part of the proof of an idea is the proof, either by experiment or by logic, of the truth of the propositions. But it is not essential that this proof of the truth should constitute the first introduction to the idea. After all, its assertion by the authority of respectable teachers is sufficient evidence to begin with. In our first contact with a set of propositions, we commence by appreciating their importance. That is what we all do in after-life. We do not attempt, in the strict sense, to prove or to disprove anything, unless its importance makes it worthy of that honour. These two processes of proof, in the narrow sense, and of appreciation do not require a rigid separation in time. Both can be proceeded with nearly concurrently. But in so far as either process must have the priority, it should be that of appreciation by use.

Furthermore, we should not endeavour to use propositions in isolation. Emphatically I do not mean, a neat little set of experiments to illustrate proposition I, and then the proof of proposition I, a neat little set of experiments to illustrate proposition II, and then the proof of proposition II, and so on to the end of the book. Nothing could be more boring. Inter-related truths are utilised *en bloc* and the various propositions are employed in any order, and with any reiteration. Choose some important applications of your theoretical subject ; and study them concurrently with the systematic theoretical exposition. Keep the theoretical exposition short and simple, but let it be strict and rigid so far as it goes. It should not be too long for it easily to be known with thoroughness and accuracy. The consequences of a plethora of half-digested theoretical knowledge are deplorable. Also the theory should not be muddled up with the practice. The child should have no doubt when it is proving and when it is utilising. My point is that what is proved should be utilised, and that what is utilised should—so far as is practicable—be proved. I am far from asserting that proof and utilisation are the same thing.

At this point of my discourse, I can most directly carry forward my argument in the outward form of a digression. We are only just realising that the art and science of education require a genius and a study of their own ; and that this genius and this science are more than a bare knowledge of some branch of science or of literature. This truth was partially perceived in the past generation ; and headmasters, somewhat crudely, were apt to supersede learning in their colleagues by requiring left-hand bowling and a taste for football. But culture is more than cricket, and more than football, and more than extent of knowledge.

Education is the acquisition of the art of the utilisation of knowledge. This

Photo: Hills and Saunders

K. S. SNELL

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is an art very difficult to impart. Whenever a textbook is written of real educational worth, you may be quite certain that some reviewer will say that it will be difficult to teach from it. Of course, it will be difficult to teach from it. If it were easy, the book ought to be burned ; for it cannot be educational. In education, as elsewhere, the broad primrose path leads to a nasty place. This evil path is represented by a book or a set of lectures which will practically enable the student to learn by heart all the questions likely to be asked at the next external examination. And I may say in passing that no educational system is possible unless every question directly asked of a pupil at any examination is either framed or modified by the actual teacher of that pupil in that subject. The external assessor may report on the curriculum or on the performance of the pupils, but never should be allowed to ask the pupil a question which has not been strictly supervised by the actual teacher, or at least inspired by a long conference with him. There are a few exceptions to this rule, but they are exceptions, and could easily be allowed for under the general rule.

We now return to my previous point, that theoretical ideas should always find important applications within the pupil's curriculum. This is not an easy doctrine to apply, but a very hard one. It contains within itself the problem of keeping knowledge alive, of preventing it from becoming inert, which is the central problem of all education.

The best procedure will depend on several factors, none of which can be neglected, namely, the genius of the teacher, the intellectual type of the pupils, their prospects in life, the opportunities offered by the immediate surroundings of the school, and allied factors of this sort. It is for this reason that the uniform external examination is so deadly. We do not denounce it because we are cranks, and like denouncing established things. We are not so childish. Also, of course, such examinations have their use in testing slackness. Our reason of dislike is very definite and very practical. It kills the best part of culture. When you analyse in the light of experience the central task of education, you find that its successful accomplishment depends on a delicate adjustment of many variable factors. The reason is that we are dealing with human minds, and not with dead matter. The evocation of curiosity, of judgment, of the power of mastering a complicated tangle of circumstances, the use of theory in giving foresight in special cases—all these powers are not to be imparted by a set rule embodied in one schedule of examination subjects.

I appeal to you, as practical teachers. With good discipline, it is always possible to pump into the minds of a class a certain quantity of inert knowledge. You take a textbook and make them learn it. So far, so good. The child then knows how to solve a quadratic equation. But what is the point of teaching a child to solve a quadratic equation? There is a traditional answer to this question. It runs thus : The mind is an instrument, you first sharpen it, and then use it ; the acquisition of the power of solving a quadratic equation is part of the process of sharpening the mind. Now there is just enough truth in this answer to have made it live through the ages. But for all its half-truth, it embodies a radical error which bids fair to stifle the genius of the modern world. I do not know who was first responsible for this analogy of the mind to a dead instrument. For aught I know, it may have been one of the seven wise men of Greece, or a committee of the whole lot of them. Whoever was the originator, there can be no doubt of the authority which it has acquired by the continuous approval which it has received from eminent persons. But whatever its weight of authority, whatever the high approval which it can quote, I have no hesitation in denouncing it as one of the most fatal, erroneous, and dangerous conceptions ever introduced into the theory of education. The

mind is never passive ; it is a perpetual activity, delicate, receptive, responsive to stimulus. You cannot postpone its life until you have sharpened it. Whatever interest attaches to your subject-matter, must be evoked here and now ; whatever powers you are strengthening in the pupil, must be exercised here and now ; whatever possibilities of mental life your teaching should impart, must be exhibited here and now. That is the golden rule of education, and a very difficult rule to follow.

The difficulty is just this : the apprehension of general ideas, intellectual habits of mind, and pleasurable interest in mental achievement can be evoked by no form of words, however accurately adjusted. All practical teachers know that education is a patient process of the mastery of details, minute by minute, hour by hour, day by day. There is no royal road to learning through an airy path of brilliant generalisations. There is a proverb about the difficulty of seeing the wood because of the trees. That difficulty is exactly the point which I am enforcing. The problem of education is to make the pupil see the wood by means of the trees.

The solution which I am urging, is to eradicate the fatal disconnection of subjects which kills the vitality of our modern curriculum. There is only one subject-matter for education, and that is Life in all its manifestations. Instead of this single unity, we offer children—Algebra, from which nothing follows ; Geometry, from which nothing follows ; Science, from which nothing follows ; History, from which nothing follows ; a couple of Languages, never mastered ; and lastly, most dreary of all, Literature, represented by plays of Shakespeare, with philological notes and short analyses of plot and character to be in substance committed to memory. Can such a list be said to represent Life, as it is known in the midst of the living of it? The best that can be said of it, is that it is a rapid table of contents which a deity might run over in his mind while he was thinking of creating a world, and had not yet determined how to put it together.

Let us now return to quadratic equations. We still have on hand the unanswered question, Why should children be taught their solution? Unless quadratic equations fit into a connected curriculum, of course there is no reason to teach anything about them. Furthermore, extensive as should be the place of mathematics in a complete culture, I am a little doubtful whether for many types of boys algebraic solutions of quadratic equations do not lie on the specialist side of mathematics. I may here remind you that as yet I have not said anything of the psychology or the content of the specialism, which is so necessary a part of an ideal education. But all that is an evasion of our real question, and I merely state it in order to avoid being misunderstood in my answer.

Quadratic equations are part of algebra, and algebra is the intellectual instrument which has been created for rendering clear the quantitative aspects of the world. There is no getting out of it. Through and through the world is infected with quantity. To talk sense, is to talk in quantities. It is no use saying that the nation is large—How large? It is no use saying that radium is scarce—How scarce? You cannot evade quantity. You may fly to poetry and to music, and quantity and number will face you in your rhythms and your octaves. Elegant intellects which despise the theory of quantity, are but half developed. They are more to be pitied than blamed. The scraps of gibberish which in their schooldays were taught to them in the name of algebra, deserve some contempt.

This question of the degeneration of algebra into gibberish, both in word and in fact, affords a pathetic instance of the uselessness of reforming educational schedules without a clear conception of the attributes which you wish to evoke in the living minds of the children. A few years ago there was an

outcry that school algebra was in need of reform, but there was a general agreement that graphs would put everything right. So all sorts of things were extruded, and graphs were introduced. So far as I can see with no sort of idea behind them, but just graphs. Now every examination paper has one or two questions on graphs. Personally, I am an enthusiastic adherent of graphs. But I wonder whether as yet we have gained very much. You cannot put life into any schedule of general education unless you succeed in exhibiting its relation to some essential characteristic of all intelligent or emotional perception. It is a hard saying, but it is true ; and I do not see how to make it any easier. In making these little formal alterations you are beaten by the very nature of things. You are pitted against too skilful an adversary, who will see to it that the pea is always under the other thimble.

Reformation must begin at the other end. First, you must make up your mind as to those quantitative aspects of the world which are simple enough to be introduced into general education ; then a schedule of algebra should be framed which will about find its exemplification in these applications. We need not fear for our pet graphs, they will be there in plenty when we once begin to treat algebra as a serious means of studying the world. Some of the simplest applications will be found in the quantities which occur in the simplest study of society. The curves of history are more vivid and more informing than the dry catalogues of names and dates which comprise the greater part of that arid school study. What purpose is effected by a catalogue of undistinguished kings and queens? Tom, Dick, or Harry, they are all dead. General resurrections are failures, and are better postponed. The quantitative flux of the forces of modern society are capable of very simple exhibition. Meanwhile the idea of the variable, of the function, of rate of change, of equations and their solution, of elimination, are being studied as an abstract science for their own sake. Not of course in the pompous phrases with which I am alluding to them here, but with that iteration of simple special cases proper to teaching.

If this course be followed, the route from Chaucer to the Black Death, from the Black Death to modern Labour troubles, will connect the tales of the medieval pilgrims with the abstract science of algebra, both yielding diverse aspects of that single theme, Life. I know what most of you are thinking at this point. It is that the exact course which I have sketched out is not the particular one which you would have chosen, or even see how to work. I quite agree. I am not claiming that I could do it myself. But your objection is the precise reason why a common external examination system is fatal to education. The process of exhibiting the applications of knowledge must, for its success, essentially depend on the character of the pupils and the genius of the teacher. Of course I have left out the easiest applications with which most of us are more at home. I mean the quantitative sides of sciences, such as mechanics and physics.

My meaning can be illustrated by looking more closely into a special case of this type of application. In my rough catalogue of the sort of subjects which should form the schedule for Algebra, I mentioned Elimination. It was not put there by accident, for it covers a very important body of thought.

In the first place, there is the abstract process of algebraic elimination for suitable simple cases. The pupil acquires a firm grasp of this by the process, inevitable in education, of working an adequate number of examples. Again, there are the graphical solutions of the same problem. Then we consider the significance in the external world. We consider the velocity, time, space, acceleration diagrams. We take uniform acceleration ; we eliminate "t" between

$$v = u + ft, \quad \text{and} \quad s = ut + \tfrac{1}{2}ft^2,$$

and eliminate " s " between

$$v^2 = u^2 + 2fs, \quad \text{and} \quad s = ut + \tfrac{1}{2}ft^2.$$

Then we remember that constant acceleration is a very special case, and we consider graphical solutions for empirically given variations of v or of f. In preference, we use those empirical formulae which occur in the pupil's experimental work. We compare the strong and weak points of the algebraic and graphical solutions.

Again, in the same connection we plot the statistics of social phenomena against the time. We then eliminate the time between suitable pairs. We can speculate how far we have exhibited a real causal connection, or how far a mere temporal coincidence. We notice that we might have plotted against the time one set of statistics for one country and another set for another country, and thus, with suitable choice of subjects, have obtained graphs which certainly exhibited mere coincidence. Also other graphs exhibit obvious causal connections. We wonder how to discriminate. And so are drawn on as far as we will.

But in considering this description, I must beg you to remember what I have been insisting on above. In the first place, one train of thought will not suit all groups of children. For example, I should expect that artisan children will want something more concrete and, in a sense, swifter than I have set down here. Perhaps I am wrong, but that is what I should guess. In the second place, I am not contemplating one beautiful lecture stimulating, once and for all, an admiring class. That is not the way in which education proceeds. No; all the time the pupils are hard at work solving examples, drawing graphs, and making experiments, until they have a thorough hold on the whole subject. I am describing the interspersed explanations, the directions which should be given to their thoughts. The pupils have got to be made to feel that they are studying something, and are not merely executing intellectual minuets.

In this connection the excellence of some of the most recent textbooks on elementary algebra emanating from members of this Association, should create an epoch in the teaching of the subject.

Finally, if you are teaching pupils for some general examination, the problem of sound teaching is greatly complicated. Have you ever noticed the zig-zag moulding round a Norman arch? The ancient work is beautiful, the modern work is hideous. The reason is that the modern work is done to exact measure, the ancient work is varied according to the idiosyncrasy of the workman. Here it is crowded, and there it is expanded. Now the essence of getting pupils through examinations is to give equal weight to all parts of the schedule. But mankind is naturally specialist. One man sees a whole subject, where another can find only a few detached examples. I know that it seems contradictory to allow for specialism in a curriculum especially designed for a broad culture. Without contradictions the world would be simpler, and perhaps duller. But I am certain that in education wherever you exclude specialism you destroy life.

We now come to the other great branch of a general mathematical education, namely Geometry. The same principles apply. The theoretical part should be clear cut, rigid, short, and important. Every proposition not absolutely necessary to exhibit the main connection of ideas should be cut out, but the great fundamental ideas should all be there. No omission of concepts such as those of Similarity and Proportion. We must remember that, owing to the aid rendered by the visual presence of a figure, Geometry is a field of unequalled excellence for the exercise of the deductive faculties of reasoning. Then of course there follows Geometrical Drawing, with its training for the hand and eye.

Photo: Flett

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But, like Algebra, Geometry and Geometrical Drawing must be extended beyond the mere circle of geometrical ideas. In an industrial neighbourhood, machinery and workshop practice form the appropriate extension. For example, in the London Polytechnics this has been achieved with conspicuous success. For many secondary schools I suggest that surveying and maps are the natural applications. In particular, plane-table surveying should lead pupils to a vivid apprehension of the immediate application of geometric truths. Simple drawing apparatus, a surveyor's chain, and a prismatic compass, should enable the pupils to rise from the survey and mensuration of a field to the construction of the map of a small district. The best education is to be found in gaining the utmost information from the simplest apparatus. The provision of elaborate instruments is greatly to be deprecated. To have constructed the map of a small district, to have considered its roads, its contours, its geology, its climate, its relation to other districts, the effects on the status of its inhabitants will teach more history and geography than any knowledge of Perkin Warbeck or of Behren's Straits. I mean not a nebulous lecture on the subject, but a serious investigation in which the real facts are definitely ascertained by the aid of accurate theoretical knowledge. A typical mathematical problem should be: Survey such and such a field, draw a plan of it to such and such a scale, and on the same linear scale construct a square of equal area. It would be quite a good procedure to impart the necessary geometrical propositions without their proofs. Then, concurrently in the same term, the proofs of the propositions would be learnt while the survey was being made.

Fortunately, the specialist side of education presents an easier problem than does the provision of a general culture. For this there are many reasons. One is that many of the principles of procedure to be observed are the same in both cases, and it is unnecessary to recapitulate. Another reason is that specialist training takes place—or should take place—at a more advanced stage of the pupil's course, and thus there is easier material to work upon. But undoubtedly the chief reason is that the specialist study is normally a study of peculiar interest to the student. He is studying it because, for some reason, he wants to know it. This makes all the difference. The general culture is designed to foster an activity of mind; the specialist course utilises this activity. But it does not do to lay too much stress on these neat antitheses. As we have already seen, in the general course foci of special interest will arise; and similarly in the special study, the external connections of the subject drag thought outwards.

Again, there is not one course of study which merely gives general culture, and another which gives special knowledge. The subjects pursued for the sake of a general education are special subjects specially studied; and, on the other hand, one of the ways of encouraging general mental activity is to foster a special devotion. You may not divide the seamless coat of learning. What education has to impart is an intimate sense for the power of ideas, for the beauty of ideas, and for the structure of ideas, together with a particular body of knowledge which has peculiar reference to the life of the being possessing it.

The appreciation of the structure of ideas is that side of a cultured mind which can only grow under the influence of a special study. I mean that eye for the whole chess-board, for the bearing of one set of ideas on another. Nothing but a special study can give any appreciation for the exact formulation of general ideas, for their relations when formulated, for their service in the comprehension of life. A mind so disciplined should be both more abstract and more concrete. It has been trained in the comprehension of abstract thought and in the analysis of facts.

Finally, there should grow the most austere of all mental qualities; I mean

the sense for style. It is an aesthetic sense, based on admiration for the direct attainment of a foreseen end, simply and without waste. Style in art, style in literature, style in science, style in logic, style in practical execution have fundamentally the same aesthetic qualities, namely attainment and restraint. The love of a subject in itself and for itself, where it is not the sleepy pleasure of pacing a mental quarter-deck, is the love of style as manifested in that study.

Here we are brought back to the position from which we started, the utility of education. Style, in its finest sense, is the last acquirement of the educated mind ; it is also the most useful. It pervades the whole being. The administrator with a sense for style, hates waste ; the engineer with a sense for style, economises his material ; the artisan with a sense for style, prefers good work. Style is the ultimate morality of mind.

But above style, and above knowledge, there is something, a vague shape like fate above the Greek gods. That something is Power. Style is the fashioning of power, the restraining of power. But, after all, the power of attainment of the desired end is fundamental. The first thing is to get there. Do not bother about your style, but solve your problem, justifying the ways of God to man, administer your province, or do whatever else is set before you.

Where then does style help? In this, with style the end is attained without side issues, without raising undesirable inflammations. With style you attain your end and nothing but your end. With style the effect of your activity is calculable, and foresight is the last gift of gods to men. With style your power is increased, for your mind is not distracted with irrelevancies, and you are more likely to attain your object. Now style is the exclusive privilege of the expert. Whoever heard of the style of an amateur painter, of the style of an amateur poet? Style is always the product of specialist study, the peculiar contribution of specialism to culture.

English education in its present phase suffers from a lack of definite aim, and from an external machinery which kills its vitality. Hitherto in this address I have been considering the aims which should govern education. In this respect England halts between two opinions. It has not decided whether to produce amateurs or experts. The profound change in the world which the nineteenth century has produced is that the growth of knowledge has given foresight. The amateur is essentially a man with appreciation and with immense versatility in mastering a given routine. But he lacks the foresight which comes from special knowledge. The object of this address is to suggest how to produce the expert without loss of the essential virtues of the amateur. The machinery of our secondary education is rigid where it should be yielding, and lax where it should be rigid. Every school is bound on pain of extinction to train its boys for a small set of definite examinations. No headmaster has a free hand to develop his general education or his specialist studies in accordance with the opportunities of his school, which are created by its staff, its environment, its class of boys, and its endowments. I suggest that no system of external tests which aims primarily at examining individual scholars can result in anything but educational waste.

Primarily it is the schools and not the scholars which should be inspected. Each school should grant its own leaving certificates, based on its own curriculum. The standards of these schools should be sampled and corrected. But the first requisite for educational reform is the school as a unit, with its approved curriculum based on its own needs, and evolved by its own staff. If we fail to secure that, we simply fall from one formalism into another, from one dunghill of inert ideas into another.

In stating that the school is the true educational unit in any national system for the safeguarding of efficiency, I have conceived the alternative system as being the external examination of the individual scholar. But every Scylla is

faced by its Charybdis—or, in more homely language, there is a ditch on both sides of the road. It will be equally fatal to education if we fall into the hands of a supervising department which is under the impression that it can divide all schools into two or three rigid categories, each type being forced to adopt a rigid curriculum. When I say that the school is the educational unit, I mean exactly what I say, no larger unit, no smaller unit. Each school must have the claim to be considered in relation to its special circumstances. The classifying of schools for some purposes is necessary. But no absolutely rigid curriculum, not modified by its own staff, should be permissible. Exactly the same principles apply, with the proper modifications, to universities and to technical colleges.

When one considers in its length and in its breadth the importance of this question of the education of a nation's young, the broken lives, the defeated hopes, the national failures, which result from the frivolous inertia with which it is treated, it is difficult to restrain within oneself a savage rage. In the conditions of modern life the rule is absolute, the race which does not value trained intelligence is doomed. Not all your heroism, not all your social charm, not all your wit, not all your victories on land or at sea, can move back the finger of fate. To-day we maintain ourselves. To-morrow science will have moved forward yet one more step, and there will be no appeal from the judgment which will then be pronounced on the uneducated.

We can be content with no less than the old summary of educational ideal which has been current at any time from the dawn of our civilisation. The essence of education is that it be religious.

Pray, what is religious education?

A religious education is an education which inculcates duty and reverence. Duty arises from our potential control over the course of events. Where attainable knowledge could have changed the issue, ignorance has the guilt of vice. And the foundation of reverence is this perception, that the present holds within itself the complete sum of existence, backwards and forwards, that whole amplitude of time, which is eternity.

GLEANINGS FROM FAR AND NEAR

" Il n'y a pas de mauvais documents "

Taine to J. E. C. Bodley.

1560. (673). Now . . . that insidious . . . bowler . . . was sticking up the Clifton batsmen every over, to finish the match with 9 wickets for 20, and a victory by 66 runs. It was a magnificent achievement, but a liberal share of the credit must go to the captain, that indomitable leader, A. N. Whitehead, who made a cricketer of himself against his native quality for the sake of the school he had so finely led in football, and who is now writing so learnedly about science and mathematics, that it is to be feared that not one of his old team would understand a word of what he says. A great captain makes a great side, and a great captain is the central figure in the umpire's first cricket recollection.—Arthur Waugh, *A Cricket Eleven*, p. 10. [Per Professor E. H. Neville.]

GREEK MATHEMATICS AND SCIENCE.*

By Sir T. L. Heath.

It is, I think, of happy augury that it should have been found possible to arrange a joint meeting of members of the local branches of the Classical and Mathematical Associations, as well as members of the Yorkshire Natural Science Association, to hear a few words about Greek science. No doubt the classical or humane studies can stand by themselves and so (in a way) can the study of science based only on good English : but I venture to think that they are both much stronger when they stand side by side and support one another. The idea that there is any necessary antagonism or opposition of interests between them is surely unfounded, and there is room for both in our system of education. It is a mistake to suppose that in our school education there is not time for the one or the other ; the question is really one of using the time that there is to the best advantage. No doubt schoolboys in the past have wasted much time in the perfunctory learning of Latin and Greek. It has often been remarked that, while a boy was more or less continuously learning Latin and Greek from the age of nine or ten till he went to the University, a girl who only began Latin and Greek at, say, fifteen or sixteen managed to catch up the boy and take a place alongside him in Part I of the Classical Tripos. It was therefore probably the inefficient or uninteresting way in which the boy used to be taught Greek and Latin that caused the waste of time. If the subjects were made sufficiently attractive and the bogey of compulsion were removed, there is no reason why the boy of good ability should not during his school years acquire a good working knowledge of both classics and science, as well as of the indispensable amount of modern languages. And, if the boy's interest can be aroused sufficiently to make him take kindly to Greek and Latin on the one side and mathematics and science on the other, I am clear that each branch will help the other and be better done.

I will address myself first to the mathematicians. Almost all the standard terms in mathematics are Greek or Latin in origin, and, although it may be possible to explain their meaning to those who do not know Greek and Latin, the knowledge thus imparted can hardly be more than parrot-knowledge. Mathematicians will surely understand all the terms better if they know them as they arise and as part of the living language of the men who invented them. Take the word *isosceles* ; a boy can be shown what an isosceles triangle is, but if he knows nothing of the derivation he will wonder why such an outlandish term, with the attendant difficulties of spelling, should be necessary to express so simple an idea. But if the mere appearance of the word when written shows him that it means a thing *with equal legs*, being compounded of ἴσος, equal, and σκέλος, a leg, he will understand its appropriateness and will have no difficulty in remembering it. The term for an equilateral triangle, on the other hand, is borrowed from the Latin, and the boy who knows Latin will take in its meaning at once ; but even this term is merely the Latin translation of the Greek ἰσόπλευρος, *equal-sided*, from ἴσος, equal, and πλευρά, a side. *Parallelogram* again can be explained to a Greekless pupil, but it will be far better understood and remembered if the boy sees in it the two words παράλληλος and γραμμή, and realises that it is a short way of expressing that the figure in question is contained by parallel lines ; and again he will best understand the word *parallel* itself if he sees in it the statement of the fact that

* A paper read, 5th March, 1921, to a joint meeting of the Classical Association (Leeds and District Branch), the Mathematical Association (Yorkshire Branch), and the Yorkshire Natural Science Association. Printed in the *Mathematical Gazette*, July, 1921.

the two straight lines so described go *alongside one another*, παρ 'ἀλλήλας, all the way. Similarly he will be saved from any vague idea (as a certain reviewer once put it) that a *rhombus* is a fish, or a *parallelepiped* some kind of prehistoric centipede, if he realises that a rhombus takes its name from its resemblance to a form of spinning-top (ῥόμβος from ῥέμβω, to spin), and that, just as a parallelogram is a figure formed by two pairs of parallel straight lines, so a *parallelepiped* is a solid figure bounded by three pairs of parallel planes (παράλληλος, parallel, and ἐπίπεδος, plane); incidentally, in the latter case, he will be saved from committing the monstrosity of writing "parallelopiped", a barbarism which has disfigured not a few textbooks of geometry.

Again, if we take up a textbook of geometry written in accordance with the most modern education circular or University syllabus and boasting complete emancipation from the hidebound rigidity of Euclid, we shall find that the phraseology used (except where made more colloquial and less scientific) is almost all pure Greek. The Greek tongue was extraordinarily well adapted as a vehicle for scientific thought; one of the characteristics of Euclid's language which his commentator Proclus is most fond of emphasising is its marvellous ἀκρίβεια, *exactness*. There is an apparent inevitableness about it which makes one wonder what would have happened if the Greeks had not invented science but left it for some one else to evolve. What people would have been capable of doing it, and what form would mathematics have taken? It is useless to speculate, because, in point of fact, mathematics *is* Greek through and through, and that is the end of the matter.

Having attempted to show why the mathematician should not neglect Greek if he is to read his subject intelligently, may I now appeal to the members of the Classical Association and show cause why classical students should not omit to study the Greek mathematicians? I well remember the days when, as an undergraduate, I first came across the *editio princeps* of the Greek text of Euclid (there was no handy Teubner edition in those days), and the thrill with which I read in the original Greek the familiar phrases of the propositions I had learnt at school. The reading of the Greek seemed to throw new light on Greek and new light on geometry at the same time. I am convinced that it would do every classical student good to read some portion at least of Euclid and Archimedes in the original. The Germans have already had the intelligence to see this. Von Wilamowitz-Moellendorff has included in his *Griechisches Lesebuch* extracts from the beginning of Euclid's *Elements*, from Archimedes, and from Heron of Alexandria. English editors of passages from the Greek for translation into English should follow this example.

But perhaps, after nearly forty years' study of the Greek mathematicians, I may have become a biased judge; in support therefore, of my thesis I will quote an independent authority. And, being a Cambridge man myself, I will go for what I want to the sister University of Oxford. Members of the Classical Association will all be familiar with the brilliant, if partly contentious, book, *The Greek genius and its meaning to us*, by one of the most enthusiastic champions of Greek in the Association. The book was reviewed in the *Oxford Magazine* of 6th March, 1913. Towards the end of his review the reviewer (I wish I knew his name) questions a remark of the author to the effect that Plato after all, "though in a thousand ways a Greek of the Greeks, in all that is most distinctive in his thought is a heretic". It may be, says the reviewer, that Plato is not direct, that he is the enemy of liberty, that he is not a humanist and that he is otherworldly. "Yet surely of all that is Greek Plato is the quintessence. To be a Greek was to seek to know; to know the primordial substance of matter, to know the meaning of number, to know the world as a rational whole. In no spirit of paradox one may say that Euclid is the most typical Greek: he would know to the bottom, and know as a rational system,

the laws of the measurement of the earth. Plato, too, loved geometry and the wonders of numbers ; he was essentially Greek, because he was essentially mathematical. It is true that he loved myths as well as mathematics ; and it would be a long story to seek to show the affinities of the one with the other. But at any rate Plato sought to know and to understand ; he demanded of every artist (be he craftsman or politician) a true knowledge of his art, and he demanded of every thinker that he should think things through and through to the Ideas which are their meaning.

" No doubt the Greek genius means many things. To one school (perhaps a prevalent school in Oxford to-day) it means an aesthetic ideal, somewhat variegated and diversified by anthropological survivals and outcrops. To others it means an austere thing, which delights in logic and mathematics ; which, continually wondering and always inquisitive, is driven by its wonder into philosophy, and into inquiry about the why and wherefore, the whence and whither, of tragedy, of the State, indeed of all things. And if one thus finds ' the Greek genius ' in Euclid and the Posterior Analytics, one will understand the motto written over the Academy—*μηδεὶς ἀγεωμέτρητος εἰσίτω*. To know what the Greek genius meant you must (if one may speak *ἐν αἰνίγματι*) begin with geometry ; and so beginning you may arrive at what the Greeks were always seeking—some knowledge of ' the question what being is ', and, incidentally, at some knowledge of the question what the being of their genius was." I am glad to take the opportunity of commending this admirable review to the earnest attention of members of the Classical Association. I have quoted from it at this length because it expresses exactly the views which I should have wished to express but in language more vivid and eloquent than I myself could command.

We naturally ask what were the special aptitudes which the Greeks possessed for science. They had, first, a love of knowledge for its own sake amounting, as Butcher says, to an instinct and a passion ; secondly, a love of truth and a determination to see things as they are ; thirdly, a remarkable capacity for accurate observation. Fourthly, while eagerly assimilating information from all quarters, from Egypt and Babylon in particular, they had an unerring instinct for taking what was worth having and rejecting the rest. As one writer has said, " it remains their everlasting glory that they discovered and made use of the serious scientific elements in the confused and complex mass of exact observations and superstitious ideas which constitutes the priestly wisdom of the East and threw all the fantastic rubbish on one side." Fifthly, they possessed a speculative genius unrivalled in the world's history.

It was this unique combination of gifts which qualified the Greeks to lead the world in all the intellectual pursuits that make life worth living.

Last, but not less important, the Greeks possessed the advantage over the Egyptians and Babylonians of having no priesthood which could monopolise learning as a preserve of its own, with the inevitable result of sterilising it by keeping it bound up with religious dogmas and prescribed and narrow routine. They had no Bible or Koran to interfere with the free use of their reasoning faculties. Their religion was just adequate to satisfy natural sentiment, but left free scope for the exercise of all their intellectual powers even when this tended to uproot and supplant religious dogmas.

Evidence of these things is furnished by the history of medicine in the earliest times. Even in Homer there is hardly a trace of superstitious rites or magic formulae being applied in the treatment of wounds ; the treatment is on rational lines ; plants and roots are used to relieve pain, while the language employed shows so much understanding of the degree of danger attaching to a particular sort of wound, and such a surprising amount of anatomical knowledge about the most important human organs, that a German medical Chief

of Staff * has actually deigned to greet the poet of the *Iliad* as a colleague. Only once (in the *Odyssey*) is a charm tried to cure haemorrhage from a wound; from this exception we may infer that the completely rational view was the prerogative of the noble and the hero, the superstitious usage in the case where it occurs betraying the bourgeois or the popular view. By the fifth century, to which period (at the latest) the Hippocratean medical writings belong, there is remarkable testimony to the complete emancipation of medicine from superstition. The term for epilepsy, which was called the *ἱερὰ νόσος*, "sacred" or "God-sent" disease, is ridiculed in uncompromising language. "Everything", says the writer, "is divine and everything is human", meaning that no one thing has a divine origin more than another. "The nature and onset of the particular disease have the same 'divine' source that everything else comes from". "These diseases appear as God-sent in precisely the same way as all others; none is more God-sent, none more human than another; . . . every one of them has a nature of its own" (*i.e.* a natural cause), "and none is without such." Again, says the author, the impostors who pretend to deal with the disease put forward the "divine" origin of it as a cloak for their inability to treat it, hoping thereby to secure themselves in either event; if the patient recovers, they claim that this result is due to their cleverness and hope to acquire a reputation accordingly; if not, their defence is easy because they can protest that the fault is not theirs but that of the gods.

The Ionian Greeks who settled in Asia Minor were the most favourably situated in respect both of natural gifts and of environment for initiating philosophy and science. They had the advantage of living in contact with two ancient civilisations, the Egyptian and Babylonian, and it was what they learnt from the Egyptians and Babylonians, elementary as it was, which supplied the first impulse and set them on the road to science.

All Greek authorities are unanimous in tracing the beginnings of geometry to Egypt. It arose out of the practical needs of the country. Revenue was raised by the taxation of landed property, and its assessment depended on the accurate fixing of the boundaries of the various holdings. The landmarks were constantly being removed by the periodical flooding due to the rising of the Nile, and it was necessary therefore to replace the landmarks, or to determine the taxable area independently of them, by the aid of an art of land-surveying. Apart from the modicum of mathematics required for the construction of pyramids, geometry in Egypt was, to judge by all the surviving documents, limited to mensuration of areas and volumes from different data, and all that we find recorded is a collection of rough practical rules for measuring, first, areas such as those of a square, a triangle, a trapezium, and a circle; and secondly, the solid content of measures of corn, etc., of different shapes. The Egyptians also constructed pyramids of a certain slope by means of arithmetical calculations. The latter involved the notion of similarity of figures, especially triangles; but the whole thing was practical and went by rule of thumb.

It required the breath of a genius like that of the Greeks to give life to such soulless forms. No doubt the inspection of figures drawn to illustrate rules of measurement for circles and plane figures would suggest to Thales, who travelled in Egypt, certain similarities and congruences, and these would set him thinking whether there were not some elementary general principles underlying the construction and relations of different figures and parts of figures. This search for general principles was in accord with the Greek instinct for generalisation and the attempt to find a rational explanation of everything.

* H. Frölich, *Die Militärmedizin Homers*. Stuttg. 1879.

Thus it was that theoretical geometry began with Thales in the first half of the sixth century B.C. The particular discoveries attributed to him are of the elementary sort which might be expected from the first founder of the science. They are, first, that a circle is bisected by any diameter; second, that the angles at the base of an isosceles triangle are equal (the famous "pons asinorum" in Euclid); third, that, if two straight lines cut one another, the vertically opposite angles are respectively equal; fourth, that, if two triangles have two angles and one side in each respectively equal, the triangles are equal in all respects. Fifthly, Thales is said to have been the first to inscribe a right-angled triangle in a circle: which must mean that he was the first to discover that the angle in a semicircle is a right angle. He also solved two problems of a practical kind: first, he showed how to measure the distance of a ship at sea; secondly, he measured the heights of pyramids by means of the shadows thrown on the ground by the pyramid and by a stick at the same moment; choosing the time when the length of the stick itself and of its shadow were equal, he argued by similarity of triangles that, at the same instant, the length of the shadow of the pyramid gave its height also.

Greek astronomy also begins with Thales. Everyone knows the story of his falling into a well when star-gazing and being rallied by a clever and pretty maidservant from Thrace (as Plato has it) for being so eager to know what goes on in the heavens that he could not see what was straight in front of him, nay, at his very feet. But he was not merely a star-gazer. There is good evidence that he predicted a solar eclipse which took place on 28th May, 585 B.C. We can conjecture the basis of this prediction. The Babylonians, as the result of observations continued through centuries, had discovered the period of 223 lunations after which eclipses recur; and Thales had probably heard of this period, either directly, or through the Egyptians as intermediaries. Thales seems further to have discovered the inequality of the four astronomical seasons; works on the Solstice and on the Equinox are attributed to him. He used the Little Bear in preference to the Great Bear as a means of finding the pole.

No one writing or speaking on Greek science should omit to notice the bold flights of the genius of Thales's immediate successor in the list of Ionian philosophers, I mean Anaximander. Anaximander wrote one treatise, *On Nature*, in which doubtless he tried to explain everything in heaven and earth. Among his strikingly original ideas the following should be mentioned. The first principle of all things he took to be, not water, or air, or fire, or any of the elements, but another sort of substance which he called by the name of the Infinite (ἄπειρον) without further definition. From this he held that the heavens are separated off and, generally, all the worlds, which are infinite in number. There are, he said, always some worlds coming into being and others passing away; they pass away into that from which they arise, "for they must pay the penalty and make reparation for the injustice they have committed, according to the sequence of time".

Anaximander boldly maintained that the earth is in the centre of the universe, suspended freely and without support, and that it remains where it is because it is equidistant from all the rest of the heavenly bodies, by which he no doubt meant that the whole universe formed a system in equilibrium, as Plato afterwards held. The earth is like a tambourine, or a short cylinder, with two circular plane faces, on one of which we live; the length of the cylinder is one third of its breadth.

The heavenly bodies are circular hoops of compressed air, opaque but containing fire inside, which shines out through vents, like gas-jets; their motion is the rotation of the hoops about their centres. It has been pointed out that this idea of the formation of tubes of compressed air in which the fire

Photo: Frank Sproston

MRS. E. M. WILLIAMS

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of each star is shut up except for the one opening is not unlike Laplace's hypothesis about the origin of Saturn's rings.

As if this were not enough, we have a suggestion about the origin of life which reminds us of Darwin. According to Anaximander animals first arose from slime evaporated by the sun ; they at first lived in the sea and had prickly coverings ; men at first resembled fishes.

Lastly, Anaximander was the first to draw a map of the inhabited earth, which, with improvements made later by Hecataeus, became an object of general admiration.

To return to the mathematical subjects. Next in succession to Thales came Pythagoras and the Pythagoreans. The name of Pythagoras was one to conjure with, and it came to be the practice to refer all Pythagorean discoveries, more or less, to the founder of the school (*αὐτὸς ἔφα, ipse dixit*). Hence it is difficult to be sure how much of the Pythagorean mathematics was due to Pythagoras himself. But there are certain things that may safely be attributed to him. To begin with astronomy. Pythagoras was the first to hold that the earth is spherical in shape, and he probably took the same view with regard to the other heavenly bodies ; a powerful motive to this was no doubt the conviction that the sphere was the most beautiful of all solid figures. Pythagoras is further said to have recognised the identity of the Morning and Evening Stars, and the fact that the sun, moon and planets have an independent movement of their own in a sense opposite to that of the daily rotation.

Arithmetic in the sense of the theory of numbers began with Pythagoras. This theory was geometrical in form, numbers being commonly represented by dots forming and filling up geometrical figures such as lines, squares, rectangles, polygons, and again solid figures such as pyramids, cubes and parallelepipeds. There is a possible explanation of this point of view. The Pythagoreans were keen astronomers, and it would probably be borne in upon them, as they continually watched the heavens, that in each constellation there are a certain number of visible stars forming a certain figure. This may account for the expression " the number in the heaven " used by the Pythagoreans, and perhaps even for their view that the whole heaven is numbers ; and again the identification of the stars with points in figures may explain the statement of Aristotle that the Pythagoreans regarded points as having magnitude.

The most wonderful discovery about numbers made by Pythagoras is that of the dependence of the musical intervals upon numerical ratios. Pythagoras discovered that, with strings at the same tension, a difference of length in the ratio of 2 to 1 gives the octave, the ratio of 3 to 2 the fifth and that of 4 to 3 the fourth. It is not surprising that, in view of connections of this sort between numbers and natural phenomena, the Pythagoreans came to liken all things to numbers and to find in the principles of numbers the principles of all things.

Coming to geometry, we find Proclus saying that Pythagoras transformed the study of geometry into a liberal education, examining the principles of the science from the beginning. That is to say that he was the first to systematise geometry as a subject in and for itself, beginning with definitions and the other necessary preliminary assumptions. Pythagoras, we are told, called geometry by the general name " inquiry ", as if implying that geometry was the whole of philosophy. There is a characteristic story that, when he returned to Samos from his travels in Egypt and tried to found a school, the Samians were indifferent, and he had therefore to take special measures to ensure that geometry should not perish at its birth. Going to the gymnasium, he sought out a well-favoured youth who seemed likely to suit his purpose and was withal poor, and bribed him to learn geometry by promising him sixpence

for every proposition that he mastered. Very soon the youth became fascinated by the subject, and Pythagoras rightly judged that the sixpenny inducement was no longer necessary. He hinted therefore that he himself was poor and must try to earn his living instead of doing mathematics ; whereupon the youth, rather than give up the study, offered to pay Pythagoras sixpence for each proposition.

Of the discoveries commonly attributed to Pythagoras the most famous is of course the theorem of Euclid I. 47, that, in any right-angled triangle, the square on the hypotenuse is equal to the sum of the squares on the other two sides. Every one knows the story that Pythagoras sacrificed an ox in celebration of this discovery. The authorities are indeed somewhat late, and all the different versions seem to be ultimately traceable to a distich by one Apollodorus (date unknown) :

> When Pythagoras discovered that famous proposition on
> the strength of which he made a splendid sacrifice of oxen.

Plutarch, it is true, expresses doubt whether the occasion of the sacrifice was the discovery of this proposition or of another, namely the solution of the problem, given two rectilineal figures, to draw a third rectilineal figure equal in area to one of the given figures and similar to the other. But the remarks of Plutarch do not suggest any doubt that Pythagoras was the discoverer of both propositions.

The Egyptians, long before Pythagoras's time, were aware that a triangle with sides in the ratio of the numbers 3, 4, 5 is right-angled, and it is believed that this and other right-angled triangles with sides in rational numbers were known in India as early as the eighth century B.C. But there is no sufficient evidence that even the Indians invented any general proof ; and, even if they did, it is probable that Pythagoras's discovery was independent. The truth of the tradition is to some extent confirmed by another which attributes to Pythagoras a general formula for finding any number of right-angled triangles with sides in the ratio of rational numbers. This was no doubt evolved through the theory of the *gnomon*, a term which must have seemed mysterious to many a generation of schoolboys reading Book II of Euclid's *Elements*. Originally meaning the upright needle of a sundial, enabling anyone to recognise (*γιγνώσκω*) or read the time of day, the word *gnomon* next came to be used of a figure in the shape of a carpenter's square, the figure which, when put round two sides of a square, makes up a larger square. The gnomon had a corresponding arithmetical signification ; it was part of the geometrical arithmetic of the Pythagoreans. Imagine the number 1, the unit, represented by a dot ; place three dots round it so as to make the four dots the corners of a square ; the three dots so added form the first gnomon. Next place five dots round two sides of the square formed by the four dots which has two for its side, and we have a square with three in its side. $1+3=4$; $1+3+5=9$; 5 is the second gnomon ; and so on. In fact, all the successive odd numbers, 3, 5, 7, etc., are gnomons, and the sum of any series of successive odd numbers beginning with 1 is a square number. Any odd number is thus the difference between some square number and the next higher integral square, that is, the square number having one more in its side : 7 is the difference between the squares 9 and 16, 11 the difference between the squares 25 and 36, and so on. It is easy by means of this fact to deduce Pythagoras's formula for finding right-angled triangles with sides in rational numbers.

Pythagoras is credited with two other discoveries, the theory of proportionals and the construction of the so-called " cosmic figures ", the five regular solids. The theory of proportionals in question was a numerical theory such as is expounded in Euclid's Book VII. Connected with proportion is the

theory of means, three of which, the arithmetic, geometric and sub-contrary (afterwards called harmonic) were known to Pythagoras.

Among the so-called " cosmic figures " is the dodecahedron, the solid with twelve equal regular pentagons as faces. The construction of this solid involves that of the regular pentagon itself, and this again depends on the solution of the problem of cutting a straight line in extreme and mean ratio (as it is called), *i.e.* so that the whole straight line has to the greater of the two segments the same ratio as the greater segment has to the lesser. This problem is solved in Prop. 11 of Euclid, Book II, and is a simple case of a general method invented by the Pythagoreans and known as the *application of areas*. The simplest case of all is that of Euclid I. 44, 45, where it is required to apply to a given straight line as base a parallelogram which shall have a given angle and shall be equal in area to any given rectilineal figure. In the more difficult cases the parallelogram to be applied has to overlap or to fall short of the given straight line, but so that the portion overlapping or the portion which is deficient has a given shape. Application pure and simple is παραβολή, application with an overlap is ὑπερβολή and application with a deficiency is ἔλλειψις. The three varieties of the process of application are used by Apollonius of Perga in finding the fundamental property of the three conic sections respectively, and this is the origin of the three names *parabola*, *hyperbola* and *ellipse* which Apollonius was the first to give to the three curves.

The Pythagoreans, then, originated two out of the three great methods running through the whole of Greek geometry, namely, those of proportion and the application of areas. Their theory of proportion, it is true, being numerical, was inadequate in that it did not apply to incommensurable magnitudes ; and hence geometrical theorems proved by it were not proved universally. But, subject to this qualification, we may say that the Pythagorean geometry covered the bulk of the subject-matter of Books I, II, IV. and VI of Euclid's *Elements*. The case is not so clear with regard to Book III of the *Elements* ; but, as the main propositions of that Book were known to Hippocrates of Chios in the second half of the fifth century B.C., we may be sure that they too were part of the Pythagorean geometry.

But the Pythagoreans also discovered the existence of incommensurable lines, or of irrational. This was no doubt first discovered with reference to the diagonal of a square, which is incommensurable with its side, being in the ratio to it of $\sqrt{2}$ to 1. The Pythagorean proof of this particular case is quoted by Aristotle ; it is by a *reductio ad absurdum* proving that, if the diagonal is commensurable with the side, it will follow that one and the same number is both odd and even. At the same time, by means of a proposition afterwards incorporated in Euclid's Book II, the Pythagoreans showed that although you can never express the ratio of the diagonal of a square to its side as the ratio of one number to another, you can obtain a series of arithmetical ratios approximating more and more closely to the ratio in question, and can carry the approximation as far as you please.

No less striking were the speculations of the Pythagoreans in astronomy. Pythagoras himself, while attributing spherical shape to the earth and the other heavenly bodies, and recognising the independent motions of the planets, seems to have retained the earth in the centre. His successors actually abandoned the geocentric theory and made the earth one of the planets moving like them round a common centre. The new system only fell short of the Copernican in that the planets did not revolve round the sun as centre, but the sun itself, like the moon, the earth and the planets, revolved about the central fire in which was supposed to reside the governing principle and the force which directs the movements and activity of the universe. The Pythagoreans also assumed another body revolving round the central fire between

it and the earth, but always in company with the earth, called the counter-earth ; this, like the central fire, was invisible to us because the hemisphere of the earth on which we live was supposed always to face outwards, away from the central fire. The order of the revolutions starting from the central fire was counter-earth, earth, moon, sun, then the five planets and, outermost of all, the sphere of the fixed stars. This made ten revolutions in all. Ten being the perfect number according to the Pythagoreans, some supposed that the counter-earth was invented solely in order to make up that number, but it is more probable that the object was to explain the frequency with which eclipses of the moon occur as compared with those of the sun.

Nor did the Pythagoreans confine themselves to mathematics and astronomy; they occupy an important place in the history of medicine. There was a flourishing school of medicine at Croton, and important discoveries were made by Alcmaeon of that place. Alcmaeon dissected animals, and discovered the most important of the sensory nerves (which, like Aristotle, he regarded as tubes or ducts) and their connection with the brain, in which he placed the centre of perceptive and intellectual life. He explained illness and disease as the disturbance of the equilibrium between the elementary opposites in the body, the cold and warm, the dry and moist, etc. In this Alcmaeon was a forerunner of Hippocrates. Alcmaeon made the separate senses the subject of close researches ; he tried to explain the fact that when we are hit in the eye we " see sparks " by the hypothesis that the eye contains fire in it. He relied for proofs upon definite evidence : about things invisible as well as about things mortal only the gods (he said) can have certain knowledge ; we, being human, must rely on inference from evidence (*τεκμαίρεσθαι*).

Two other natural philosophers should before now have been mentioned for definite contributions to science, Anaxagoras (500–428 B.C.) and Empedocles (494–434 B.C.). Anaxagoras discovered that the moon receives its light from the sun ; the moon according to him is made of earth, the sun is a red-hot stone larger than the Peloponnese. He had original ideas on the evolution of the universe. Assuming a rapidly rotating mass in the centre, he conceived that the violence of the whirling motion caused portions to be detached and hurled off into space ; these hurlings-off are very like those by which Kant and Laplace accounted for the formation of the solar system. Anaxagoras also gave a tolerably accurate explanation of the risings of the Nile. Empedocles deserves mention for his theory that light travels, taking time to pass from one point to another. Aristotle mentions the theory, but dismisses it as too bold an assumption.

Nor should I omit to mention the great name of Democritus. Of his doctrine of atoms a Frenchman has remarked that " None of the ideas which antiquity has bequeathed to us has enjoyed a greater or even an equal triumph ". Democritus may indeed be called the father of Physics. In mathematics he was no less distinguished, as we shall see later on.

Of other subjects I am not qualified to speak in detail. But on the biological side I ought to refer to Aristotle who was the creator of scientific zoology and comparative anatomy.* Further, his pupil Theophrastus was the first to write systematic treatises on botany. Two elaborate works of his on plant lore and plant physiology respectively enable us to claim that the Greeks founded the science of botany also.

In speaking of mathematics I have confined myself to the Elements. I should have liked to say something of the higher geometry of the Greeks: how, in the century from 450 to 350 B.C., when the Elements were not yet

* Cp. L. C. Miall, *Early Naturalists*, p. 3, " Aristotle is the real founder of Comparative Anatomy, and perhaps no science ever made so prosperous a start, enriched from its birth with such a multitude not only of facts but of ideas."

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fully established, they had advanced beyond the Elements, and solved three famous problems in higher geometry, the squaring of the circle, the trisection of any angle, and the duplication of the cube ; how Eudoxus, a pupil of Plato, discovered, first, the great theory of proportion incorporated by Euclid in his Book V and, secondly, the " method of exhaustion " (as it is called) by means of which Eudoxus was able to measure the content of pyramids and cones and to deal with the areas of circles and the volumes of spheres ; how Archimedes, using and extending Eudoxus's method, measured curvilinear areas and solids by a procedure which, though purely geometrical, is equivalent to the integral calculus ; how, again, in astronomy Heraclides of Pontus, a pupil of Plato, discovered that the earth rotates on its own axis and that Venus and Mercury revolve round the sun like satellites, and how finally Aristarchus of Samos, about 25 years earlier than Archimedes, put forward the heliocentric hypothesis, thereby completely anticipating Copernicus. All this, however, would make a long story and would perhaps be unsuitable to a mixed audience.

What I should like now to emphasise is the character of the Greek contribution to science which makes it a possession for all time. To put it shortly, the Greeks created science, and mathematics in particular, by laying down principles, fixing terminology and inventing the methods *ab initio* ; and they did this with such certainty that in the centuries which have followed there has been no need to go back on anything essential or to unlearn anything that they taught.

On the principles no one could be clearer than Aristotle. Every demonstrative science, he says, has to do with three things, the subject-matter, the things proved and the things from which the proof starts (*ἐξ ὧν*). It is not everything that can be proved, otherwise the chain of proof would be endless ; you must begin somewhere, and you must start with things admitted but indemonstrable. These are, in the first place, principles common to all sciences which are called *axioms* or *common opinions*, as that " of two contradictories one must be true ", or " if equals be subtracted from equals the remainders are equal " ; secondly, principles peculiar to the subject-matter of the particular science, say, geometry. First among the latter principles are definitions : there must be agreement as to what we mean by certain terms. Secondly, we must assume the existence of certain things, *e.g.* in geometry of points and lines, in arithmetic of the odd and even. Thirdly, we must assume certain other things which are less obvious and cannot be proved but yet have to be accepted, at least in the first instance, on the *ipse dixit* of the teacher only ; these are called *postulates*, because they make a demand on the faith of the learner. Euclid's Postulates are of this kind, especially the fifth, known as the parallel-postulate.

Definitions began with Pythagoras. Plato gave much attention to them. It is clear that certain axioms and postulates were formulated before Aristotle's time. When therefore we come to the definitions, postulates and axioms or " common notions " in Euclid, we have a collection which is largely traditional, and only in a few cases, as in that of the definition of a straight line, and that of the parallel-postulate, can we be fairly certain that they were due to Euclid himself. It is however in Euclid that terminology is fixed for the first time ; in Plato and Aristotle it was still somewhat fluid. The first terms for things geometrical were based on physical resemblances to common objects ; *e.g.* an *angle* was *γλωχίς*, the word for an arrow-head, *surface* was *χροιά*, colour or skin, *perpendicular* was a straight line drawn *gnomon-wise* (*κατὰ γνώμονα*), i.e. like the perpendicular needle of a sundial, *point* was *στιγμή*, a *puncture*. The terms became more abstract in course of time : *σημεῖον*, a mark, replaced *στιγμή* as the name for a point, *surface* became *ἐπιφάνεια*, signifying what is

apparent to the eye. Certain terms which had at first a more general signification became restricted in use: *e.g.* τετράγωνον is still used in Aristotle for any figure having four angles (as the name implies), *i.e.* a quadrilateral; with Euclid it definitely becomes a *square*. And so on.

We come now to methods and form of exposition. Take a proposition of Euclid and compare it with a proposition in a modern mathematical textbook: if there is any difference, it is that, if anything, the old Alexandrian was more completely rigorous than the writer of to-day. In a proposition of Euclid there were definite subdivisions, afterwards called by specific names, which were arranged in strictly logical order. There was first the *enunciation* (πρόταοις); secondly, the *setting-out* (ἔκθεσις), *i.e.* the statement of the precise data, a given straight line, two triangles or whatever they might be; thirdly, the so-called διορισμός, literally *definition* or *delimitation*, which is a statement of what we are required to do or to prove, not in general terms as in the enunciation, but with reference to the particular data in the setting-out; then, fourthly, comes the *construction* (κατασκευή), the addition, where necessary, of more lines, etc., to the figure; fifthly, the *proof* (ἀπόδειξις), and sixthly, the *conclusion* (συμπέρασμα), stating what has actually been done, which in general follows the wording of the original enunciation. In the case of a problem it often happens that a solution is not possible unless the particular data are such as to satisfy certain conditions; in this case there is yet another constituent part in the proposition, namely a statement of the condition or conditions of possibility, which was called by the same name διορισμός, delimitation, as that applied to the third constituent part of a theorem.

Now this elaboration in the arrangement of propositions was not the work of Euclid alone. It was the result of practical evolution of logical method. The earliest complete textbook that we possess is by Autolycus, one of Euclid's immediate predecessors. The propositions in it are precisely in the Euclidean form. Earlier still are certain proofs preserved by later writers; one is by Archytas (nearly a century before Euclid), others are contained in the fragments from Hippocrates of Chios (second half of fifth century B.C.) preserved by Eudemus, some more are Pythagorean; in all these cases the form of proof is essentially the same, showing that it goes back to the Pythagoreans, if not to Pythagoras himself.

The technical terms for particular methods were probably invented later, after the methods themselves had become established.

One method of solution was the *reduction* of one problem to another. This was called ἀπαγωγή, a term which seems to occur first in Aristotle. But instances of such reduction occurred long before. Hippocrates of Chios reduced the problem of duplicating the cube to that of finding two mean proportionals in continued proportion between two straight lines, that is, he showed that, if the latter problem could be solved, the former was thereby solved also; and it is probable that there were still earlier cases in the Pythagorean geometry. Next, there is the method of mathematical *analysis*. The invention of this is specifically attributed by Proclus and Diogenes Laertius to Plato; but this is more than doubtful, because like *reduction* (to which it is closely akin) analysis in the mathematical sense must have been in use much earlier. *Analysis* and its correlative *synthesis* are carefully defined by Pappus: "in analysis we assume that which is sought as if it were already done and we inquire what it is from which this results, and again what is the antecedent cause of the latter, and so on, until by so retracing our steps we come upon something already known or belonging to the class of principles. But in synthesis, reversing the process, we take as already done that which was last arrived at in the analysis, and by arranging in their natural order as consequences what were before antecedents and successively connecting them one

with another we arrive finally at the construction of that which was sought."

Again, the method of *reductio ad absurdum* is a variety of analysis. We start from a hypothesis, namely the contradictory of that which we desire to prove: then we use the same process of analysis, carrying it back until we arrive at something admittedly false or absurd. Aristotle describes this method in various ways as *reductio ad absurdum*, proof *per impossibile*, or proof leading to the impossible. But here again, though the term was new, the method was not. Aristotle in fact illustrates it by reference to the ancient (probably Pythagorean) proof that the diagonal of a square is incommensurable with its side: the proof shows (as we said) that, if the diagonal were commensurable with the side, it would follow that one and the same number is both odd and even, which is absurd.

The story of Greek mathematics to the time of Archimedes and Apollonius is a record of an almost continuous series of capital discoveries following each other in rapid succession, and of successful attacks upon one problem after another, each more difficult than the one before, culminating within about three centuries from the first beginnings. But it must not be supposed that it was all plain sailing: far from it. The genius of the Greek for clear thinking and carrying everything through to its logical conclusion showed itself just as much in criticism as in construction; in criticism too they seem to have "thought of everything", as Aristotle says of Democritus. Greek mathematics in fact passed through the fire. So accustomed were mathematicians to having to defend themselves against criticism that they had a technical term for the objections which were or might be raised to their demonstrations; ἔνστασις was the word. There is at least one proposition, or rather a part of a proposition, in Euclid which, according to Proclus, was inserted for no other purpose than to enable an objection to be met which might be raised against a later proposition on the ground that it appeared to take account of one case only where two cases were possible.

The first great crisis through which Greek geometry passed was the result of the discovery of the incommensurable by the Pythagoreans. This invalidated the proofs of all theorems about magnitudes incommensurable with one another which used the old theory of proportion invented by the Pythagoreans, since that theory, being a numerical theory, applied only to commensurable magnitudes. That method of proof had therefore to be abandoned in such cases in favour of some other not depending on proportions. It is probable that many proofs were so remodelled. Although, when this could be done, the propositions were rehabilitated, great inconvenience was caused and a slur cast on geometry generally. No wonder that the discovery of the incommensurable was supposed to have been kept secret for a time and that death by shipwreck was said to have punished the first person who divulged it. The scandal was not finally removed till Eudoxus established his great theory of proportion which was applicable to incommensurable and commensurable magnitudes alike.

Closely connected with the difficulty of the incommensurable was that of continuity and the infinite divisibility of continuous magnitudes. Democritus stated the dilemma in a striking form in his question about successive sections of a cone parallel to the base. Suppose, he said, you have a section of a cone parallel to and (as we should say) indefinitely close to the base—the very next section to the base, so to speak—what are we to say of the section? Is it equal to the base or not equal to it? If it is equal, the next section will likewise be equal to the same and the cone will really be, not a cone at all, but a cylinder. On the other hand, if the next section to the base is unequal to the base and in fact less, and if the next succeeding section is again less, the cone will have indentations on its surface like steps, which is very absurd.

Even more piquant were the paradoxes of Zeno which have just as much point to-day as they had twenty-three centuries ago, and which (if we are to believe Mr. Bertrand Russell) were never even properly understood till recent years. Aristotle is said to have called Zeno the inventor of dialectic, and no wonder. His four puzzles took the form of proving by *reductio ad absurdum* that motion is impossible. They are complementary to one another; two are based on the hypothesis that time and space are divisible *ad infinitum*, two on the hypothesis that both time and space are made up of indivisible elements, and both hypotheses are shown to lead to the same result, that motion is impossible. In other similar arguments which are handed down by Simplicius, Zeno held that, if divisibility is without limit, it must end, not in the infinitely small (because that is by hypothesis still divisible), but in *nothing*. But, if you reverse the process and add nothing to itself for ever and ever, you will never produce any finite magnitude at all. This proof that the infinitesimal or infinitely small does not exist had a profound influence on the later developments of mathematics. Antiphon the Sophist, for instance, had said that, if you inscribed successive regular polygons in a circle, continually doubling the number of sides, you would sometime arrive at a polygon the sides of which would coincide with the circumference of the circle. Warned by Zeno, mathematicians denied this and substituted the statement that, by continuing the construction you could inscribe a polygon approaching equality with the circle as nearly *as you please*. Similarly, in all cases they avoided the expressions infinitely great and infinitely small, confining themselves to saying that a magnitude could be increased until it exceeded any given finite magnitude of the same kind however great, and conversely that a magnitude could be diminished by subtraction of half or more than half, then of half or more than half the remainder, until there was left a magnitude smaller than any assignable magnitude of the same kind however small.

In the time of the Sophists mathematics had its ups and downs. Some of them, like Hippias of Elis, were distinguished in mathematics and taught it regularly. Others would have none of it. Plato represents Protagoras as saying "The other Sophists maltreat the young, for at an age when the young have escaped from the arts, they take them against their will and plunge them once more into the arts, teaching them the arts of calculation, astronomy, geometry and music"—and here he cast a glance at Hippias—"whereas, if any one comes to me, he will not be obliged to learn anything except what he comes for." Protagoras too attacked the mathematicians on their own ground; for he wrote a book on mathematics, περὶ τῶν μαθημάτων, and argued, against the geometers, that no such straight lines or circles as they assume exist in nature and that, *e.g.*, a material circle does not in fact touch a straight line at one point only. We hear of a title of a book by Democritus "On a difference of opinion (if that is the right reading) or on the contact of a circle and a sphere"; and it is tempting to suppose that the object of the book was to refute attacks on geometry such as that of Protagoras. Aristotle too has much the same kind of criticism in mind when he says, "It is untrue to assert, as some do, that a geometer's hypotheses are false because he assumes that a line which he has drawn is a foot long when it is not, or straight when it is not straight. The geometer bases no conclusion on the particular line being what he has assumed it to be; he argues about what it represents, the figure being merely an illustration."

The Epicureans and Sceptics objected to the whole of mathematics; the basis of their objection was almost entirely the opposition between the fundamental hypotheses of mathematics, the point, the line, the surface, etc., and the data of sense-perception. There are no such things (they said) as mathematical points, lines, etc. Even if points exist, you cannot make up a line out

of points. It is absurd to define a line as that which, if it is turned about one of its extremities, will always touch a plane ; a line being, say, length without breadth, and therefore an unsubstantial thing, cannot be turned round at all. And so on.

Lastly, there were the critics who, without objecting to the subject-matter or the methods of mathematics, objected that the assumptions made did not cover the whole ground, in other words, that the proofs contained *tacit* assumptions, which were not admissible without some sort of argument. The very first proposition of the first Book of Euclid was assailed on this ground by Zeno of Sidon. Euclid draws an equilateral triangle by joining the two ends respectively of the given straight line to the point in which the two circles cut one another. The proof that this gives an equilateral triangle is not conclusive (said Zeno) unless it be first assumed that neither two straight lines nor two circles can have a portion common to both. Suppose for instance that the two straight lines meet before they reach the point of intersection of the circles ; then we have, not an equilateral triangle, but an isosceles triangle with the two equal sides less than the base and a bit of a straight line standing up at the top like a flagstaff. Even Zeno's criticism was taken so seriously that Posidonius wrote a whole book to controvert him. Thus it was that Greek mathematics, having had its procedure checked and controlled at every point, became the beautiful and perfect thing that we find it.

There is a story that King Ptolemy asked Euclid for a short cut to geometry, and that Euclid replied that " in geometry there is one road for all ". It is surely true to say that there is only one road to science and that it was the Greeks who found it.

1561. (23). Professor MacCullagh. In Strabane he was, while very young, placed at the only respectable school at that time in the town. Here his genius soon displayed itself. After school hours he was almost constantly employed in solving mathematical problems ; yet, it is remembered that when Euclid was first put into his hands he was dissatisfied with the task. He was only required to get the solution of a problem by heart, like a copy of verses, and repeat it. There was no attempt made at explanation. This did not suit the character of his mind, which even then could not rest until it thoroughly understood the nature of everything that came before it. For some days he was restless, unhappy and puzzled, wandering about with his Euclid in his hand. In his perplexity he met a neighbour, a working carpenter, a man of cleverness and talent, who, seeing the boy evidently unhappy, was good enough to ask him what was the matter. He immediately told his good-natured friend that he was obliged to get by heart a set of strange words, the meaning of which he wanted to understand, at the same time showing him the proposition he was committing to memory for the next day's task. The carpenter instantly sat down with the puzzled boy, and in a short time showed him what a proof was. This was the way in which Professor MacCullagh first learned to prove a proposition in Euclid.—*Obituary Notices*, Royal Society, 1847 ; *Phil. Mag.*, March 1848, p. 222.

THE CASE AGAINST THE MATHEMATICAL TRIPOS.*

By G. H. Hardy.

My address to-day is the result of an informal discussion which arose at our meeting last year after the reading of Mr. Bryon Heywood's paper. You may remember that Mr. Heywood put forward a number of suggestions, with whose general trend I found myself entirely in sympathy, for the improvement of the courses in higher pure mathematics in English universities. He did not criticise one university more than another; but Cambridge is admittedly the centre of English mathematics, so that it is almost inevitable that such suggestions should be considered from the Cambridge standpoint; and that, if my recollections are correct, is what actually happened in the discussion.

My own contribution to the discussion consisted merely in an expression of my feeling that the best thing that could happen to English mathematics, and to Cambridge mathematics in particular, would be that the Mathematical Tripos should be abolished. I stated this on the spur of the moment, but it is my considered opinion, and I propose to defend it at length to-day. And I am particularly anxious that you should understand quite clearly that I mean exactly what I say; that by "abolished" I mean "abolished", and not "reformed"; that if I were prepared to co-operate, as in fact I have co-operated in the past, in "reforming" the Tripos, it would be because I could see no chance of any more revolutionary change; and that my "reforms" would be directed deliberately towards destroying the traditions of the examination and so preparing the way for its extinction.

There are, however, certain possible grounds of misunderstanding which I wish to remove before I attempt to justify my view in detail. The first of these is unimportant and personal, but probably I shall be wise if I refer to it and deal with it explicitly. Our proceedings here do not as a rule attract a great deal of attention, but they are occasionally noticed in the press; and the writer of a well-known column in an evening paper, who was inspired last year to comment on these particular remarks of my own, observed that it was unnecessary to take such iconoclastic proposals seriously, since Cambridge mathematicians were very unlikely to be disturbed by the criticisms of an Oxford man. Perhaps, then, I had better begin by stating that the Mathematical Tripos is an institution of which I have an extensive and intimate knowledge. It is true that I have not taken any part in it during the last six years; but I was a candidate in both parts of it, I took my degree on it, I have examined in it repeatedly under both the old regulations and the new, and, when the old order of merit was abolished in 1910, I was a secretary of the committee which forced this and other changes through a reluctant Senate. I am not then a mere jealous outsider, itching to destroy an institution which I cannot comprehend, but a critic perfectly competent to express an opinion on a subject which I happen to know unusually well.

The second possible misapprehension which I am anxious to remove is decidedly more important. It is possible that some of you may have come here expecting me to deliver a general denunciation of examinations; and if so I am afraid that I shall disappoint you. Denunciation of examinations, like denunciation of lectures, is very popular now among educational reformers, and I wish to say at once that most of what they say, on the one topic and on the other, appears to me to be little better than nonsense. I judge such denunciations, naturally, as a mathematician; and it has always seemed to

* Presidential Address to the Mathematical Association, 1926. Printed in the *Mathematical Gazette*, March 1926.

me that mathematics among all subjects is, up to a point, the subject most obviously adapted to teaching by lecture and to test by examination. If I wish to teach twenty pupils, for example, the exponential theorem, the product theorem for the sine, or any of the standard theorems of analysis or geometry, it seems to me that by far the best, the simplest, and the most economical course is to assemble them in a lecture-room and explain to them collectively the essentials of the proofs. It seems to me also that, if I wish afterwards to be certain that they have understood me, the obviously sensible way of finding out is to ask them to reproduce the substance of what I said, or to apply the theorems which I proved to simple examples. In short, up to a point, I believe in formal lectures, and I believe also in formal examinations.

There are in fact certain traditional purposes of examinations, purposes for which they always have been used, and for which they seem to me to be the obvious and the appropriate instrument. There are certain qualities of mind which it is often necessary to test, and which can be tested by examination much more simply and more effectively than in any other way. If a teacher wishes to test his pupils' industry, for example, or their capacity to understand something he has told them, something perhaps of no high order of difficulty, but difficult enough to require some little real intelligence and patience for its appreciation, it seems to me that his most reasonable course is to subject them to some sort of examination. Examinations have been used in this manner, from time immemorial, in every civilised country ; there are, in England, quite a number of large, elaborately organised, and, so far as I can judge, quite sensibly conducted examinations of this type ; and with such examinations I have no sort of quarrel.

There are, however, in England, and, so far as I know, in no other country in the world, a number of examinations, of which the Mathematical Tripos at Cambridge, and Greats at Oxford, have been the outstanding examples, which are of quite another type, and which fulfil, or purport to fulfil, quite different and very much more ambitious ends. These examinations originate in Oxford and Cambridge, and are found in their full development there only, though they have been copied to a certain extent by our modern universities. They are described as " honours " examinations, and pride themselves particularly on their traditions and their " standards ". To these examinations are subjected a heterogeneous mass of students of entirely disparate attainments, and the examination professes to sort out the candidates and to label them according to the grade of their abilities. Thus in the old Mathematical Tripos there were three classes, each arranged in order of merit, while in the new there are three classes and two degrees of marks of special distinction. It is evident that such an examination is not content with fulfilling the ends which I have admitted that an examination can fulfil so well ; it is not, and prides itself that it is not, merely a useful test of industry, intelligence, and comprehension. It purports to appraise, and it must be admitted that to some extent, though very imperfectly, it does appraise, higher gifts than these. A " b^* " in the Tripos, or a first in Greats, is taken to be, and in a measure is, an indication of a man quite outside the common run. It is these examinations and these only, these examinations with reputations and standards and traditions, which seem to me mistaken in their principle and useless or damaging in their effect, and which I would destroy if I had the power. An examination can do little harm, so long as its standard is low.

I suppose that it would be generally agreed that Cambridge mathematics, during the last hundred years, has been dominated by the Mathematical Tripos in a way in which no first-rate subject in any other first-rate university

has ever been dominated by an examination. It would be easy for me, were the fact disputed, to justify my assertion by a detailed account of the history of the Tripos, but this is unnecessary, since you can find an excellent account, written by a man who was very much more in sympathy with the Tripos than I am, in Mr. Rouse Ball's *History of Mathematics in Cambridge*. I must, however, call your attention to certain rather melancholy reflections which the history of Cambridge mathematics suggests. You will understand that when I speak of mathematics I mean primarily pure mathematics, not that I think that anything which I say about pure mathematics is not to a great extent true of applied mathematics also, but merely because I do not want to criticise where my competence as a critic is doubtful.

Mathematics at Cambridge challenges criticism by the highest standards. England is a first-rate country, and there is no particular reason for supposing that the English have less natural talent for mathematics than any other race ; and if there is any first-rate mathematics in England, it is in Cambridge that it may be expected to be found. We are therefore entitled to judge Cambridge mathematics by the standards that would be appropriate in Paris or Göttingen or Berlin. If we apply these standards, what are the results? I will state them, not perhaps exactly as they would have occurred to me spontaneously—though the verdict is one which, in its essentials, I find myself unable to dispute—but as they were stated to me by an outspoken foreign friend.

In the first place, about Newton there is no question ; it is granted that he stands with Archimedes or with Gauss. Since Newton, England has produced no mathematician of the very highest rank. There have been English mathematicians, for example Cayley, who stood well in the front rank of the mathematicians of their time, but their number has been quite extraordinarily small ; where France or Germany produces twenty or thirty, England produces two or three. There has been no country, of first-rate status and high intellectual tradition, whose standard has been so low ; and no first-rate subject, except music, in which England has occupied so consistently humiliating a position. And what have been the peculiar characteristics of such English mathematics as there has been? Occasional flashes of insight, isolated achievements sufficient to show that the ability is really there, but, for the most part, amateurism, ignorance, incompetence, and triviality. It is indeed a rather cruel judgment, but it is one which any competent critic, surveying the evidence dispassionately, will find it uncommonly difficult to dispute.

I hope that you will understand that I do not necessarily endorse my friend's judgment in every particular. He was a mathematician whose competence nobody could question, and whom nobody could accuse of any prejudice against England, Englishmen, or English mathematicians ; but he was also, of course, a man developing a thesis, and he may have exaggerated a little in the enthusiasm of the moment or from curiosity to see how I should reply. Let us assume that it is an exaggerated judgment, or one rhetorically expressed. It is, at any rate, not a *ridiculous* judgment, and it is serious enough that such a condemnation, from any competent critic, should not be ridiculous. It is inevitable that we should ask whether, if such a judgment can really embody any sort of approximation to the truth, some share of the responsibility must not be laid on the Mathematical Tripos and the grip which it has admittedly exerted on English mathematics.

I am anxious not to fall into exaggeration in my turn and use extravagant language about the damage which the Tripos may have done, and it would no doubt be an extravagance to suggest that the most ruthless of examinations could destroy a whole side of the intellectual life of a nation. On the

other hand it is really rather difficult to exaggerate the hold which the Tripos has exercised on Cambridge mathematical life, and the most cursory survey of the history of Cambridge mathematics makes one thing quite clear; the reputation of the Tripos, and the reputation of Cambridge mathematics stand in correlation with one another, and the correlation is large and negative. As one has developed, so has the other declined. As, through the early and middle nineteenth century, the traditions of the Tripos strengthened, and its importance in the eyes of the public grew greater and greater, so did the external reputation of Cambridge as a centre of mathematical learning steadily decay. When, in the years perhaps between 1880 and 1890, the Tripos stood, in difficulty, complexity, and notoriety, at the zenith of its reputation, English mathematics was somewhere near its lowest ebb. If, during the last forty years, there has been an obvious revival, the fortunes of the Tripos have experienced an equally obvious decline.

Perhaps you will excuse me if I interpolate here a few words concerning my own experience of the Tripos, which may be useful as a definite illustration of part of what I have said. I took the first part of the Tripos in 1898, and the second in 1900: you must remember that it was then the first part which produced wranglers and caught the public eye.

I am inclined to think that the Tripos had already passed its zenith in 1898. There had already been one unsuccessful attempt to abolish the order of merit, a reform not carried finally till 1910. When the first signs of decline might have been detected I cannot say, but the changes in the Smith's Prize examination, and the examination for Trinity Fellowships, must have been partly responsible, and these had been determined by dissertation for a considerable time. At any rate it was beginning to be recognised, by the younger dons in the larger colleges, and to some extent by undergraduates themselves, that the difference of a few places in the order of merit was without importance for a man's career. This, however, is comparatively unimportant, since it is less the examination itself than its effect on teaching in the university that I wish to speak of now.

The teaching at Cambridge when I was an undergraduate was, of course, quite good of its kind. There were certain definite problems which we were taught to solve; we could learn, for example, to calculate the potential of a nearly spherical gravitating body by the method of spherical harmonics, or to find the geodesics on a surface of revolution. I do not wish to suggest that the two years which I spent over the orthodox course of instruction—my second two years were occupied in a different way—were altogether wasted. It remains true that, when I look back on those two years of intensive study, when I consider what I knew well, what I knew slightly, and of what I had never heard, and when I compare my mathematical attainments then with those of a continental student of similar abilities and age, or even with those of a Cambridge undergraduate of to-day, it seems to me almost incredible that anyone not destitute of ability or enthusiasm should have found it possible to take so much trouble and to learn no more. For I was indeed ignorant of the rudiments of my profession. I can remember two things only that I had learnt. Mr. Herman of Trinity had taught me the elements of differential geometry, treated from the kinematical point of view; this was my most substantial acquisition, and I am grateful for it still. I had also picked up a few facts about analysis, towards the end of those two years, from Prof. Love. I owe, however, to Prof. Love something much more valuable than anything he taught me directly, for it was he who introduced me to Jordan's *Cours d'analyse*, the bible of my early years; and I shall never forget the astonishment with which I read that remarkable work, to which so many mathematicians of my generation owe their mathematical

education, and learnt for the first time as I read it what mathematics really meant.

It has often been said that Tripos mathematics was a collection of elaborate futilities, and the accusation is broadly true. My own opinion is that this is the inevitable result, in a mathematical examination, of high standards and traditions. The examiner is not allowed to content himself with testing the competence and the knowledge of the candidates; his instructions are to provide a test of more than that, of initiative, imagination, and even of some sort of originality. And as there is only one test of originality in mathematics, namely the accomplishment of original work, and as it is useless to ask a youth of twenty-two to perform original research under examination conditions, the examination necessarily degenerates into a kind of game, and instruction for it into initiation into a series of stunts and tricks. It was in any case certainly true, at the time of which I am speaking, that an undergraduate might study mathematics diligently throughout the whole of his career, and attain the very highest honours in the examination, without having acquired, and indeed without having encountered, any knowledge at all of any of the ideas which dominate modern mathematical thought. His ignorance of analysis would have been practically complete. About geometry I speak with less confidence, but I am sure that such knowledge as he possessed would have been exceedingly one-sided, and that there would have been whole fields of geometrical knowledge, and those perhaps the most fruitful and fascinating of all, of which he would have known absolutely nothing. A mathematical physicist, I may be told, would on the contrary have received an appropriate and an excellent education. It is possible; it would no doubt be very impertinent for me to deny it. Yet I do remember Mr. Bertrand Russell telling me that he studied electricity at Trinity for three years, and that at the end of them he had never heard of Maxwell's equations; and I have also been told by friends whom I believe to be competent that Maxwell's equations are really rather important in physics. And when I think of this I begin to wonder whether the teaching of applied mathematics was really quite so perfect as I have sometimes been led to suppose.

I remember asking another friend, who was Senior Wrangler some years later, and has since earned a very high reputation by research of the most up-to-date and highbrow kind, how the Tripos impressed him in his undergraduate days, and his reply was approximately as follows. He had learnt a little about modern mathematics while he was still at school, and he understood perfectly while he was an undergraduate, as I certainly did not, that the mathematics he was studying was not quite the real thing. But, he continued, he regarded himself as playing a game. It was not exactly the game he would have chosen, but it was the game which the regulations prescribed, and it seemed to him that, if you were going to play the game at all, you might as well accept the situation and play it with all your force. He believed—and remember, if you think him arrogant, that his judgment was entirely correct—that he could play that game at least as well as any of his rivals. He therefore decided deliberately to postpone his mathematical education, and to devote two years to the acquisition of a complete mastery of all the Tripos technique, resuming his studies later with the Senior Wranglership to his credit and, he hoped, without serious prejudice to his career. I can only add—lost as I am in hopeless admiration of a young man so firmly master of his fate—that every detail of these precocious calculations has been abundantly justified by the event.

I feel, however, that I am laying myself open at this point to a challenge which I shall certainly have to meet sooner or later, and which I may as well deal with now. It will be said—I know from sad experience that such things

are always said—that I am applying entirely wrong criteria to what is after all an examination for undergraduates. I shall be told that I am assuming that the principal object of the Cambridge curriculum is to increase learning and to encourage original discovery, and that this is false ; that learning and research are admirable things, but that a great university must not allow itself to be overshadowed by them ; and that, in short, a German professor of mathematics, however universal his reputation and profound his erudition, is not necessarily the noblest work of God. Indeed, at this point I seem to hear the voice of my opponent grow a little louder, as he points out to me that I am entirely misconceiving the function of an English university, that the universities of England are not at all intended as machines for the generation of an infinite sequence of professors, but as schools for the development of intellect and character, as training grounds of teachers, civil servants, statesmen, captains of industry and proconsuls, in short as nurseries where every young Englishman may learn to add his quotum to the fulfilment of the destinies of an imperial race. I wish very heartily, I confess, that I was not going to be told all this, but I know very well that it is coming, for have I not heard it all a hundred times already, and did we not hear it all in 1910, from all the Justices who had been wranglers in their day?

Perhaps, however, I shall not be wasting your time entirely if I occupy a few minutes in an attempt to examine this indictment as dispassionately as I can. I find it very difficult to believe that most of the quite considerable body of quite intelligent people who continue to use this kind of language at the present day, and to turn it to the defence of our present university education, can have considered at all coolly some of the implications of what they say. On the other hand I recognise that it is a good deal easier to laugh at these people than to refute them, and that, if I were to attempt a reasoned reply to their contention, considered as a general principle to guide us in the construction of an educational system, then I should have a long and tiresome argument before me.

Fortunately, this is unnecessary. We are not now discussing educational systems generally, but the merits of a particular examination. We have not to undertake a general defence of mathematics and the position which is at present allowed to it in education, or to repel the very formidable onslaught which might be directed against it by Philistinism pure and simple. You and I and the Justices are after all agreed in wanting to see some sort of education in higher mathematics, and differ only in the kind of mathematical education which we prefer. The question is merely whether it is possible to defend the Mathematical Tripos on these lines, and we can appeal here, I think, to the method of *reductio ad absurdum*.

I have already put forward one test of a mathematical education, namely that it should produce mathematicians, as " mathematician " is understood by the leading mathematicians of the world ; and this test, whatever its defects may be, has one merit at any rate, namely that it is clear and sharp and easily applied. It is also a test to which I suppose that everybody would agree in attaching *some* degree of importance, since it must be extraordinarily difficult for any English mathematician to maintain that it is of no importance whatever whether English mathematics be good or bad. The question therefore is not of the validity of the test, but only of its relative importance.

Now there is one obvious difference between my test, which I will call for shortness the professional test, and the slightly more orotund test which I have tried to state in general terms. My test has certainly this advantage, that I am testing a mathematical education as a means to one of the ends which a mathematical education may reasonably be expected to secure, and which it is hardly possible to secure in any other way. When, on the other

hand, we attempt to test a training in higher mathematics, the highest such training the country offers, by its effects generally on the intelligence and character of those who submit to it, we are at once confronted with a question which is obviously more fundamental, whether intelligence and character of the type at which we aim are really developed very effectively by a training in higher mathematics. And as we are all mathematicians here, we need not indulge in humbug about it. We know quite well that the answer is No.

It is hardly likely that anybody here will accuse me of any lack of devotion to the subject which has after all been the one great permanent happiness of my life. My devotion to mathematics is indeed of the most extravagant and fanatical kind ; I believe in it, and love it, and should be utterly miserable without it, and I have never doubted that for any one who takes real pleasure in it and has a genuine talent for it, it is the finest intellectual discipline in the world. I believe also that a fair knowledge of mathematics is, even for those who have no pronounced mathematical talent, extremely useful and extremely stimulating, and that it should be part of the ordinary intellectual capital of all intelligent men. I am prepared indeed to go further, since I believe that a very large proportion of students abandon mathematics merely because it is often very badly taught, and might push their mathematical studies a good deal further than they do at present with very great profit to themselves. But I do not believe for a moment, and I do not believe that the majority of competent mathematicians believe, that the intensive study of higher mathematics, whether it be understood as it would be in a foreign university, or whether it be understood as it has in the past been understood in the Mathematical Tripos, forms a good basis of a general education. I am not at all sure that, among all possible subjects which might be selected as special courses of study, for an intelligent young man of no particular talent, mathematics is not the worst. Indeed, I think that this is being gradually recognised both by teachers of mathematics and by students themselves, and that it is for this reason that the Mathematical Tripos is more and more becoming, and rightly becoming, the special preserve of professional mathematicians. And if this be so, then surely it is quite obviously futile to judge the Tripos by anything but a professional standard.

It seems to me, then, that the opponents of the professional standard are committed from the beginning to a very paradoxical position, and yet it seems —such is the attraction of a paradox—that they are actually dissatisfied with its already sufficiently serious difficulties and determined to surround it by still more fantastic entrenchments. For they generally go on to maintain that mathematics may indeed be made the finest of intellectual disciplines, but only if it is taught in a manner which ignores or rejects every development of recent years. It will teach you to think, so long as you are not allowed to think quite correctly ; it will widen your interests and stimulate your imagination, so long as you are carefully confined to problems in which mathematicians have lost interest for fifty years. In a word, the mathematics of the amateur is all right, and that precisely because it is so much more than a little wrong, but if we once allow mathematics to be dominated by the professionals, that is to say by the men who live in the subject and are familiar with its vital developments, then its energy will be sapped and its educational efficacy destroyed. And of all insane paradoxes, surely, this is one of the most portentous.

I have told you already that I am not much of a believer in the general educational efficacy of a specialised mathematical training. I do not believe that it is possible to build a character or an empire on a foundation of mathematical theories ; but surely it must be still more impossible to build either on a foundation of Tripos problems. If I were compelled to undertake so

crazy an enterprise, I would select the true theorems rather than the false, the fundamental facts of mathematics rather than its trivial excrescences, the problems which are alive to-day rather than those which perished in the mid-Victorian era.

I would suggest to you, then, that, when you have next to listen to the mathematical reactionary who laments the good old days, if you doubt your competence to judge for yourself the merits of his complaints, you should apply to what he says Hume's test of the greater improbability. It does not seem very likely that the modern experts are all wrong, but it is quite possible. It is also possible that the times have really left a conservatively-minded mathematician a little bit behind ; that his lectures and his textbooks have run out of date ; that there is a good deal in modern mathematics which he finds it too great an effort to master ; and that it gives him a good deal less trouble to abuse the modern tendencies than to repair the gaps in his own mathematical equipment. This also is, of course, extremely improbable ; but you must ask yourself which is the greater improbability of the two.

I do not propose to waste further time on the discussion of this question : in what more I have to say about the Tripos I shall adopt a frankly professional view. I shall judge the Tripos by its real or apparent influence on English mathematics. I have already told you that in my judgment this influence has in the past been bad, that the Tripos has done negligible good and by no means negligible harm, and that, so far from being the great glory of Cambridge mathematics, it has gone a very long way towards strangling its development. There are further questions to consider. We may ask in the first place, if it be granted that what I have said about the past is roughly true, how far have things improved? Is it not true already that the Tripos means a great deal less, and English mathematics appreciably more, than forty years ago, and is it not extremely likely that, even if there be no further radical changes, this process will continue ? Then, if we are not content to answer this question by a simple affirmative and leave it there, we may ask what really are the fundamental faults of an examination on the Tripos model, and whether it is not possible to make less drastic suggestions for its improvement.

I began my address with what was to a certain extent a defence of examinations. I said that under certain conditions I believed in examinations, that is to say in examinations of a sufficiently lowly type, which do not profess to be more than a reasonable test of certain rather humdrum qualities. The phrases which I used were vague, and I ought no doubt to attempt to define my own standard a little more precisely. This is naturally not quite easy, but I will risk some sort of definition. I should say, roughly, that the qualities which I have in mind—reasonable industry, reasonable intelligence, reasonable grasp—would be about sufficient to carry a candidate, in any of the orthodox Oxford or Cambridge examinations, into a decent second class. Beyond that, I do not believe in recognising differences of ability by examination.

I said this here last year, and I was at once challenged. I was asked, whatever could you do, if you could not tell the quality of a man by looking at his examination record? I wonder whether my questioner realised that these elaborate honours examinations, so far from being one of the fundamental necessities of modern civilisation, are a phenomenon almost entirely individual to Oxford and Cambridge, copied in a half-hearted fashion by other English universities, and, beyond that, having hardly a parallel in the world? Does Germany suffer from intellectual stagnation, because there are no honours examinations in her universities? Germany does not think in terms of first and seconds ; we think in terms of them, so far as we do so think, and perhaps the practice is to some extent abating, merely because we have heard so

much about them that they have become to us like bitter ale or eggs and bacon, and we have forgotten that we could get on quite happily without them.

I remember, if you will excuse my referring once more to the forgotten controversies of 1910, a curious saying of, I think, Mr. Justice Romer. Mr. Justice Romer circulated a flysheet to the Senate, deploring, of course, the proposal to abolish the Senior Wrangler. " What ", he asked judicially, " is the function of the Tripos? " and he replied, " Surely to examine and to make distinctions between young men." It would indeed be difficult to compress a larger quantity of vicious educational doctrine into a smaller number of words. The exactly opposite doctrine, that no distinctions should be made by examination except such as practical necessities may make imperative, is surely somewhere a little nearer to the truth.

Let us then consider, with the view of meeting the objection which was raised to what I said last year, whether the kind of distinctions made by the Mathematical Tripos are, in fact, of any particular practical utility. The evidence of ability provided by the Tripos is as follows. A candidate may obtain a first, second, or a third. He may obtain a mark—the " b " mark—of adequate knowledge of some special subject, or a higher mark—the " b^* " mark—of special distinction in that subject. The test case for us, and the only one I have time to consider now, is the highest mark. When a candidate has attained this mark, what has he gained?

In the first place, he has gained the natural feeling of satisfaction which everyone experiences when he is adjudged to have performed a definite task at least as well as anybody else. He will feel with pleasure and pride that the world is rewarding honest work, and these entirely creditable feelings may spur him on to further effort. Has he gained anything of more tangible or permanent value?

A man who can attain the highest honours in the Tripos is generally a good enough mathematician to hope for a permanent academic career. How far will his " b^* " assist him along this career? Will anyone give him a position, a fellowship or a lectureship, on the strength of it? If he thinks that, he will be very quickly disillusioned.

It is possible that there are positions, in the junior grades of the teaching staffs of certain universities, which are sometimes filled on the strength of an examination record. I have never come across such a post myself, but it is probable enough that they exist. Academic positions are usually bestowed, not on examination record, but from personal knowledge or on the strength of private recommendations from competent people. I have taken part myself in many such appointments. When applications are invited, the testimonials submitted by candidates contain statements of their academic qualifications, and often of their performance in examinations, and it would be an exaggeration to say that such records are never referred to. There are usually a fair proportion of the candidates whose qualifications seem obviously below the standard expected, and a glance at their examination records often provides useful evidence in confirmation of this view. I do not remember any case of any other kind in which such a record has played any part in the decision, or has been referred to in the discussion by any member of the board of electors.

I suppose that this is generally understood, and that candidates for such positions are not usually under any delusion about the attention paid to the records which they submit. It may, however, be urged that an examination record of high distinction might often determine the decision if the post were of a less purely academic kind, for example if it were a mastership in one of the big public schools. It may be so, but I must confess that—at any rate

in the particular case which we are considering—I am uncommonly sceptical about it. In the first place, people who obtain "b^*"s have usually scientific ambitions, and the last thing they want is a mastership in the most historic of public schools. It is not possible now for the richest or the most aristocratic school to obtain a really distinguished mathematician, even if it wants one, which of course in general it does not. Finally, even if the demand existed and the supply were there, the headmasters of the great public schools do not, so far as my experience has shown me, select their assistants in this way, but proceed much more in the spirit of a board of electors, though naturally in a more capricious and autocratic way.

My conclusion, then, is that the highest certificate of merit offered by the Tripos might just as well be scrapped, for all the influence it exerts on the careers of those who obtain it. I suppose, in fact, that the universities, and most of the other bodies in whose hands educational patronage is vested, have come in practice to very much the same conclusion as my own, that examinations are an admirable test of competence and industry, but ineffective and erratic as a test of any higher gift. The Government stands alone, so far as I know, in attaching a definite money value to an examination class, and even the Government stops short of rewarding the only mark which could plausibly pretend to be a mark of real distinction.

If such distinctions are in effect futile, why should we waste our time and our energies in making them, even if we were certain that they do no harm? If Einstein had taken the Mathematical Tripos, what would it matter what place he took? The world can recognise its Einsteins quickly enough when it gets the opportunity. If Einstein sits for an examination, let him have his degree, assuming that he can satisfy the examiners. What is the object of taking all these pains to make to-day, uncertainly and half-heartedly, distinctions which, if they have any foundation in reality, the world will make in its own much sharper fashion to-morrow?

I have left to the last the defence of the Tripos which I find myself most difficult to meet. It is a defence difficult to overcome, because it proceeds on what a chess-player would call close lines. This defence, which I have often heard from mathematicians whose judgment I value, and which I wish to treat with all respect, is simply this: that the examination has already been considerably relaxed, and that the effects of its relaxation can already be traced in a corresponding strengthening of English mathematics; that it may be indefensible in principle, but that the spirit of emulation which it fosters may conceivably do some slight positive good; and that, now that so many of its teeth have been successfully drawn, it is not very obvious that it does any very serious harm. This is undeniably the case for the Tripos in its strongest and sanest form.

I should admit that, up to a point, the defence is sound. I would go so far as to admit that the system now does little harm to men of what I may define roughly as fellowship standard. The truth is that the principles for which I am contending have been so far recognised that a man of this degree of ability need not really disturb himself very seriously about the examination. Such a man may pursue a course of serious mathematical study with every confidence that, unless he is wilfully neglectful, he can obtain without any intensive effort all such honours as the Tripos can bestow. The test, in short, does him no harm, because for him it has lost its meaning. This I admit, and, of course, I recognise that it is a very large admission, since it destroys a good deal of the case which could be urged so irresistibly against the Tripos thirty years ago on strictly professional grounds. It is no longer true that Cambridge is notably behind the times, or that its courses compare particularly unfavourably, at any rate in the subjects about which I am best qualified

to judge, with those at any but the very best of continental universities. This, I think, Mr. Heywood did not recognise sufficiently; it was the only point in his address from which I particularly dissented. It is no longer true that the development of a decent school of English mathematics is being steadily throttled by the vices of its principal examination.

We must recognise this and rejoice that things have moved so far, and if they have moved just because the glamour of the Tripos has faded, we shall only rejoice the more. We need not rush to the conclusion that the whole case against the Tripos has been destroyed. We have to think of its effects, not only on students of the highest class, but also upon teachers of mathematics in the university, and upon students a little less gifted than those of whom I have spoken hitherto. I am afraid that it is still true that mathematical teaching is hampered very seriously by the examinations, both in Cambridge and in Oxford, where the system is different in detail, but in essentials the same.

In the first place, it is still true that a large proportion of students, either wilfully, because they exaggerate the importance of the examination, or from ignorance, because they have never heard of anything better, or (and I am afraid that this is the most common explanation) because they are driven to it by tutors who have to justify themselves in the eyes of college authorities greedy for firsts, for one or other of these reasons allow their mathematical education to be stunted by absorption in examination technique. They spend hour after hour, which ought to be devoted to lectures or reading, in working through examination papers, or the collections of problems in which English textbooks are so rich, exhausting themselves and their tutors in the struggle to turn a comfortable second into a marginal first. It is possible that the effect of all this mistaken exertion is more directly damaging to the tutors than to the students themselves; but a pupil cannot draw much inspiration from a tutor who is always tired, and there is hardly a tutor in Oxford, and not very many in Cambridge, who has not about twice as much teaching as any active mathematician should be asked to undertake. A professor at Oxford or Cambridge is very much his own master, but even a professor may be handicapped very seriously in his teaching by the recollection of the syllabus of the schools. It is often very hard to ask your pupils to go on listening to you when you know that what you tell them will gain them no credit in an examination to which they attach enormous importance and over which you have practically no control. I should always like to ignore the examination completely, and often summon up the courage to do so for a while, only to be pulled up short a few weeks later by the thought that after all it is hardly fair.

Whatever, then, may be said about the improvement of Cambridge teaching, and however much the dominion of the Tripos may have abated of its rigours of thirty years ago, I adhere to the view which I expressed to you last year, that the system is vicious in principle, and that the vice is too radical for what is usually called reform. I do not want to reform the Tripos, but to destroy it. And if you ask me whether the Tripos is a peculiar case, or whether what I have said applies to all other high-grade honours examinations, I can only answer that, so far as I can see, it does. The Tripos is the worst case. It is the oldest examination and the most famous, and generally the most strongly entrenched; and mathematics is a subject in which it is particularly easy to examine ferociously, so that the evils of the system stand out here in the clearest light. But, of course, the greater part of what I have said about the Tripos could be applied with almost equal force to Greats.

I wish, then, to abolish the Tripos, and as I know perfectly well that neither I nor anyone else will succeed in doing so, since the practical difficulties would

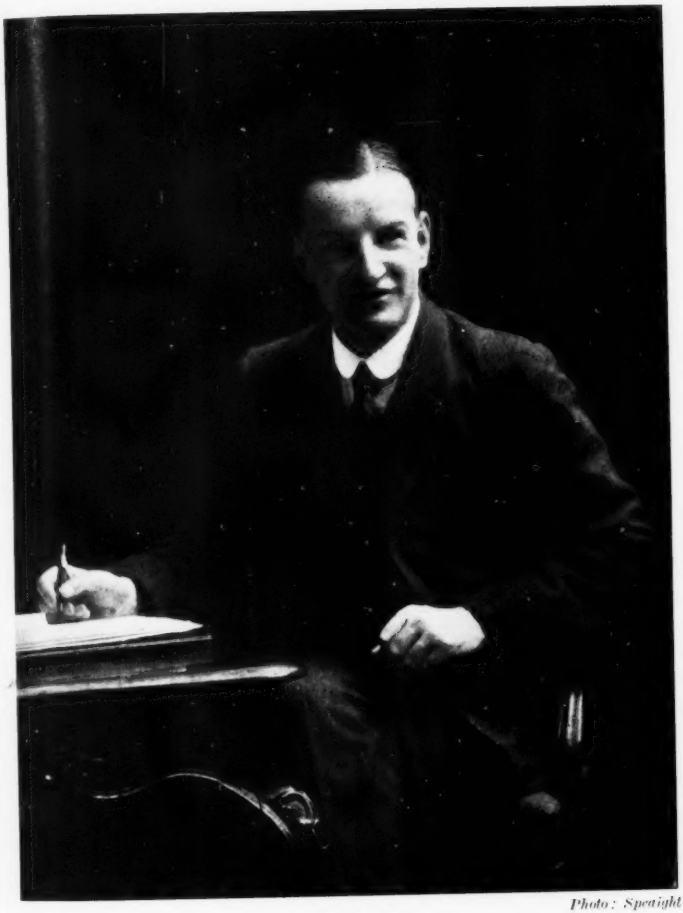

Photo: Speaight

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be so serious, and the force of tradition is so terribly strong, I may reasonably be asked what seem to me the best practical steps for a more moderate reformer to take. You will probably have inferred from my remarks that I am not prepared with any very illuminating suggestions. I could, of course, suggest many changes of detail, both in the schedules and in the conduct of the examination ; but my suggestions would be comparatively unimportant ; and I should not be prepared to expend my energies in pressing them, since they would all be inspired by the same ideal, and that an ideal whose realisation will no doubt remain hopeless for many years. Indeed I am afraid that my advice to reformers might sound like a series of stupid jokes. I should advise them to let down the standard at every opportunity ; to give first classes to almost every candidate who applied ; to crowd the syllabus with advanced subjects, until it was humanly impossible to show reasonable knowledge of them under the conditions of the examination. In this way, in the course of years, they might succeed in corrupting the value of the prizes which they have to offer, and in all probability time would do the rest.

1562. (796). The Master of Trinity College, Dr. Whewell, was unfortunate enough, five and twenty years ago, to fall into one of Nature's traps. . . . In his work on Mechanics, he happened to write *literatim* and *verbatim*, though not *lineatim*, as follows :

There is no force, however great,
Can stretch a cord, however fine,
Into a horizontal line,
Which is accurately straight.

. . . A little before the occurrence of the preceding, Prof. Woodhouse, in his treatise on Astronomy, was more unfortunate than Mr. Whewell ; for he only made the first half of a stanza—and left the undergraduates to add the second. . . . He was then superintending . . . the completion of the Observatory, which was to be his own official residence ; and some dissatisfaction had been expressed at the expense of ornamenting the grounds. So, between them, Woodhouse and the wags made the following :

If a spectator
Be at the equator,
At the point represented by A :
So says Mr. Woodhouse,
Who lives in the good house
For which other people must pay.

(There was) an older notice of a stanza, from Smith's " Optics ", which has not yet found its other half . . .

If parallel rays
Come contrary ways
And fall upon opposite sides. . . .

—M., *The Athenaeum*, 1846, p. 842. (Obviously De Morgan.)

1563. (161). Definition of Infinity : the place where things happen that don't. [Per Mr. W. Hope-Jones.]

REVIEWS.

Lectures on Ten British Mathematicians of the Nineteenth Century. By ALEXANDER MACFARLANE. First Edition. First Thousand. Pp. 148. 5s. 6d. net. (Chapman and Hall.)*

The present volume, we are delighted to hear, is the first instalment of twenty lectures given at Lehigh University some years before the author's lamented death. Macfarlane belongs to the little band of men who were proud to own allegiance to their teacher Tait, and he will be best known to a large number of mathematicians as a founder, as an industrious and active official, and ultimately as President of the *International Association for Promoting the Study of Quaternions*.

The present instalment of these lectures is confined to studies of ten pure mathematicians: Peacock, De Morgan, William Rowan Hamilton, Boole, Cayley, Clifford, H. J. S. Smith, Sylvester, Kirkman and Todhunter. Of De Morgan we have the *Memoirs* published by his wife Sophia in 1882. We have the definitive life of Hamilton by Robert Perceval Graves. Frederic Pollock contributed a charming prefatory notice to the collection of Clifford's *Lectures and Essays* published in 1879; and H. J. S. Smith's appreciatory introduction to the *Scientific Papers* (1882) is well known. We might have expected a biography of Boole from the pen of his gifted wife, but a certain instability of temperament, which persisted after the guiding hand of her husband was withdrawn, unfortunately prevented this. For the rest, as far as we are aware, we must go in the main to short notices attached to editions of *Collected Works*, or to the obituaries in the *Proceedings* of such bodies as the Royal Society, the London Mathematical Society, or the Royal Astronomical Society. In the case of Kirkman, indeed, the dozen or so pages in this volume will be to a great extent a revelation, and we have met with many mathematicians who were barely aware of his existence. Of Todhunter we have three very characteristic papers in the *Cambridge Review*, vol. v. (1883–4), in which, however, we seem to see far more of the picturesque personality of the writer, dear old "Johnny Mayor", than of the subject of his discourse, the vivid touches it contains reminding us of Johnson's phrase on "reading Shakespeare by flashes of lightning". Of Todhunter, now that Besant is gone, there will be few who can say, as Professor Clifton writes to us, "I was introduced to Boole in his rooms". There are many still with us whose intimacy with some of the others would entitle them to write upon such attractive subjects. But we have said enough on the point to show that for the most part we are grateful for what the author has written upon these great figures in the mathematical history of early and mid-Victorian times.

It is interesting to note that Peacock's abilities were unsuspected in his early life. Such reputation as he had was due to his daring feats of climbing rather than to any promise in the class-room. Cayley was a mountaineer and Clifford an incomparable gymnast. In 1869 the latter writes from Cambridge: "I am at present in a very heaven of joy because my corkscrew was encored last night at the assault of arms; it consists in running at a fixed upright pole, which you seize with both hands and spin round and round, descending in a corkscrew fashion." A friend writes: "I am appalled now to think that he climbed up and sat on the cross bars of the weather-cock on a church tower, and when by way of doing something worse I went up and hung by my toes to the bars he did the same." The rest of the ten were not athletes, though Sylvester's use of the sword-cane was on one occasion dexterous enough to send him back from America to England without waiting

* Printed in the *Mathematical Gazette*, October, 1917.

to pack his books (p. 108). De Morgan was an exquisite flautist ; Kirkman formed out of the roughest material " a parish choir of boys and girls who could sing at sight any four part song put before them ". Todhunter knew two tunes, " one was ' God save the Queen ', the other wasn't. The former he recognised by the people standing up."

Among the men of his time at Cambridge, Peacock stood next in reputation to Whewell and Sedgwick. After a preliminary sketch of the conditions obtaining in the England of the opening of the nineteenth century, the lecturer traces the part played by Peacock in the work of reform, and indicates the manner in which, by " silent perseverance only ", he and his colleagues in the new movement which they inaugurated hoped to " reduce the many-headed monster of prejudice and make the University answer her character as the loving mother of good learning and science ". The quotation from the preface of Frend's *Principles of Algebra* (and here for *in*tractable read *un*tractable, for **soluble** read *solvable*, for *coulor* read *labour*) gives an idea of the gulf that was fixed between the British and the continental mathematicians. His published work is of considerable intrinsic and of great historical interest, and it is certainly worth while considering whether it will not be well to reprint his article on " Arithmetic " from the *Encyclopaedia Metropolitana*, which De Morgan graced with the epithet " most valuable ", and to which he paid the " most undoubted compliment which can be paid to any work " (*Arithmetical Books*, pp. xvii, 91). And there is also a masterly report to the meeting of the British Association of 1833 which should be rescued from an undeserved oblivion. Of De Morgan there is not much that is new, but all will endorse the recommendation of Tait to Macfarlane : " If you want to read something entertaining get De Morgan's *Budget of Paradoxes* out of the library." And the advice was taken to such effect that the lecturer dubs it De Morgan's " most " unique work!

Bishop Brinkley, the quondam astronomer, of whom Lord Norbury remarked that he had to thank his stars for his episcopal rank, said, so Maria Edgeworth tells us : " Young Mr. Hamilton may be a second Newton ", which is a slight variant from Mr. Macfarlane's less comprehensive : " This young man, I do not say will be, but is, the first mathematician of his age." Sedgwick's estimate of the great Irishman was even higher—that Hamilton possessed within himself powers and talents perhaps never before combined within one philosophical character. The epitaph Hamilton desired for himself was *ἀνὴρ φιλόπονος καὶ φιλαλήθης*. As might be expected from a pupil of Tait, the importance attached to Hamilton's *Quaternions* by the author overshadows his work in other fields. It is interesting to remember that Cayley, while a pupil at the bar, went over to Dublin, and sat next to Salmon at Hamilton's first course of lectures on Quaternions. Salmon's appreciation of his companion, twelve years before the death of Cayley (*Nature*, 20th Sept., 1883) is well worth reading for many reasons. We may select three " nuggets ", one of which has escaped Mr. Macfarlane. The tripos list of 1842 was placed in Cayley's hands when on the top of the coach on a night journey to London, but he waited for the morning light to discover that he was the Senior Wrangler of that year.* He applied to Mr. Christie to be numbered among the pupils of that eminent conveyancer, modestly suppressing any mention of the fact

* Another instance of real or assumed indifference of this kind is that of Dean Cowie, Senior Wrangler (1839) in a great Johnian year, the first four Wranglers, Cowie, Frost, Colson, Reyner, all being from St. John's. Cowie was one of the few Senior Wranglers the foundation of whose later distinction was laid by French hands. He drove up to the Senate House in a dog-cart, and enquired from his seat of the crowd : " Who's at the top? " To the answer : " Cowie! " he replied with the utmost nonchalance : " I thought he would be," and drove on.

of his antecedents, and only after some cross-examination did Christie find that he had before him a Senior Wrangler and a Fellow of Trinity. And last, " He is a good linguist, and, as was said of Möltke, there are few European languages in which he does not know how to hold his tongue." Cayley " was invaluable as a mathematical referee ". We hope to print some day in " Gleanings Far and Near " one of his reports, picked up by chance from its precarious enshrinement in a *Salmon's Conics* on a bookstall. Mr. G. B. Mathews refers (*Nature*, 17th May, 1917) to Cayley's Presidential Address to the British Association of 1883, remarking that from the lecturer it receives " a proper amount of attention ". We hope to reproduce in the *Gazette* from time to time many purple passages from addresses such as this, which are buried in reports and the like, unlikely to be disinterred but by those who have large libraries within reach.

The anagram referred to on p. 89 was signed " West ", which stood for We, S(tewart) T(ait), and it is to be found in *Nature*, 15th Oct., 1874. The reply was composed by Clifford, except a page or two at the beginning, " at a single sitting which lasted from a quarter to ten in the evening till nine o'clock the following morning ". Clifford's frankness and vigour of speech were naturally " offensive to many who held views as to religion in which he could not share ", and reference might have been made to the somewhat cruel caricature which resulted from this in his later days when he figured as " Mr. Saunders " in W. H. Mallock's *New Republic*.

Of the ten pure mathematicians, Smith was, as Mr. G. B. Mathews says, the Admirable Crichton. In the obituary notice written by Spottiswoode (*Nature*, 22nd Feb., 1883) the following passage is quoted from a letter of Huxley: " Henry Smith impressed me as one of the ablest men I ever met with ; and the effect of his great powers was almost whimsically exaggerated by his extreme gentleness of manner, and the playful manner in which his epigrams were scattered about. They were so bright and sharp that they transfixed their object without hurting him. I think that he would have been one of the greatest men of our time, if he had added to his own wonderfully keen intellect and strangely varied and extensive knowledge the power of caring very strongly about the attainment of any object ", and Spottiswoode suggests that Smith's care for the attainment of an object was measured " rather by his estimate of its ultimate value than by its present advantage ". His papers were remarkable for their exquisite finish, and Clifford's remark on reading a paper by Hesse has more than once been applied to the work of H. J. S. Smith: " this is reading poetry ". The reader may add to the account of the 3000 francs prize (pp. 99, 105) that the motto he adopted for his paper was:

> Quotque, quibusque modis possint in quinque resolvi
> Quadratos numeri pagina nostra docet.

The story is also told in *Nature*, 12th April, 1883, pp. 538 and 564. Curiously enough the three—Spottiswoode, Huxley and H. J. S. Smith—were simultaneous recipients of honorary degrees at Cambridge in 1879 ; the Public Orator's speeches are given in full in *Nature*, 19th June, of that year.

The lines quoted in this lecture (p. 98) occur in Clerk Maxwell's address to Section A of the British Association, 1870, and it would be interesting to know their source :

> " Where never creeps a cloud or moves a wind,
> Nor ever falls the least white star of snow,
> Nor ever lowest roll of thunder moans,
> Nor sound of human sorrow mounts, to mar
> Their sacred everlasting calm."

C. T. DALTRY

facing page 148

Huxley again appears, this time in the lecture on Sylvester, in connection with the moment when for once the great master of polemic met his match. In the pages on linkages, the Peaucellier mechanism for drawing a straight line is mentioned. Eight years after Sylvester's death, and three years after this lecture was delivered, attention was drawn by Mr. G. T. Bennett (*Phil. Trans.*, 1905, vi. S. ix. p. 803) to the fact that Peaucellier had been anticipated by Sarrut, one of his own countrymen, who approached the problem by a different method (*Comptes Rendus*, vol. 36, 1853, p. 1036). Attention might have been called to the fertility of Sylvester's imagination as shown in his invention of appropriate terms in the course of his researches (did he not call himself a very Noah in this respect?). It has been suggested that the most felicitous of all was the use of " alternants " in the theory of determinants.

Macfarlane refers to the fact that Sylvester prided himself on his poetical powers, but does not allude to his Latin verse. Thanks to the kindness of Mr. A. O. Prickard, " Novi Collegii socius, vir amicissimus tam mitis ac benevolens quam lepidus et eruditus ", and of Mr. P. E. B. Jourdain, we have before us a *Corolla Versuum*, for private circulation, with, needless to say, a revised edition with numerous additional notes, and dedicated : Cantatrici eximiae | nec minus habili ad delineandum manu | in omni genere artium verae Atheniensi | a Professore Saviliano Geometriae apud Oxonienses | et eodem Novi Collegii Socio | summisse oblata. Idibus Octob : 1895.

QUOD FELIX FAUSTUMQUE SIT.

We give one epigram :

URBI ET ORBI.

Lond. : 1865.

Quum theorema Newtoni de radicibus imaginariis equationum per longos annos non demonstratum, demonstrassem.

Ortae ex Cartesio, quam Newtonus insuper auxit,
Doctrinae en! demum fons et origo patent.

which is Englished thus, thirty years later :

TO OXFORD AND THE WORLD.

Sprung from Descartes, which Newton to a higher level bore,
See now the fount and source lie open of that lore.

Or, again, Descartes' and Newton's law lay hid in night :
Heaven touched my heart with fire, and all was light.*

And a footnote : Ut lacus Nili ab ortus loco ita *fons* ab *origine* differt ; idcirco *patet* non *patet* iudicio meo legere fas est. Hoc distichon quum ex memoria excidisset (nam publicatum fuit in fasciculo inscripto (*Proceedings of London Mathematical Society*, anno 1865), revocavi admonitus ab Admirali Jonquières dum mense Augusto huius anni inter socios Instituti Franciae convivarer.

The writer of this notice possesses Kirkman's own copies of Plücker's *System der analytischen Geometrie* (1835), *System der Geometrie des Raumes* (1846) and the *Theorie der algebraischen Curven* (1839). These are bound in one volume, and from the inscriptions we see that he became possessed of the first in 1848, and of the second in the next year. He apparently had a habit of annotating his books, for the most part in a shorthand, possibly of his own. The volume contained two drafts of a relic not without its interest, a letter sent to a correspondent whose name, as the journalist says, does not transpire. The longer of the two is as follows :

* The last is a memory from Pope's epitaph :

" Nature and Nature's laws lay hid in night ;
God said, ' Let Newton be ' ; and all was light."

Croft Warrn, July 8, 1851.

My dear Sir,

As you have no books, I send you my slight improvement of Plücker's way of proving the number of double tangents.

Take any curve of n^{th} order $F(x, y) = 0 = U$ and diffg,

$$F(x, y) = 0 = U \text{ and diffg,}$$
$$F' + F_1 y' = 0,$$
$$F_1 y'' + F'' + 2F_1' y' + F_{11} y'^2 = 0.$$

Then $$F_1^2 F_{11} - 2F' F_1 F_1' + F_1^2 F'' = 0 = V,$$

a curve of $3n - 4$ degree, is the locus of all the points of U for which $y'' = 0$ or the rad. cur. is infinite. If these points are at a finite distance they must be points of inflection on U. The number of these is just $n \,.\, \overline{3n-4} - \phi n$, $\frac{1}{2}\phi n$ being the number of asymptotes common to V and U; now $\phi n = 4$ when $n = 2$, since the points of inflection are nil. ϕn is $\therefore$ either $2n$ or $n+2$; but ϕn is always even, $\therefore$ it is $2n$; and the points of inflection are $n \,.\, \overline{3n-6}$ for $U = 0$.

The number of tangents w you can draw to U from any point $w \parallel$ to any line is $n \,.\, \overline{n-1}$. These lines are determined by the equations

$$y' = \alpha,$$
$$F = 0 = U,$$
$$F' + F_1 y' = 0 = W,$$

and you draw the lines from your point to the $n \,.\, \overline{n-1}$ intersections of U and W; but this supposes that F, F', F_1 are not $= 0$ together. If they are, the curve U has so many double points, and since V and dV equal 0 in this case, each of which makes two intersections of U and W, and reduces the possible proper tangents by 2. That is, the *class* of U is reduced by 2 for every double point on U. The order m of $R = 0$, the polar reciprocal curve of U, is equal to the class of $U = 0$, and $v \,.\, v$. Further, the number of tangents possible to draw is reduced by 3 for every cusp on U; for if the oval or loop vanishes, it becomes a cusp : now the line through the double point represents a pair of tangents practicable from p, and when the loop vanishes, it will represent 3 of them. Every point of inflection on U makes a cusp on R. The class of R is $m \,.\, \overline{m-1}$, or $n \,.\, \overline{n-1}(n \,.\, \overline{n-1} - 1)$, but this is reduced by the $3n(n-2)$ cusps of R by $3 \,.\, 3n \,.\, \overline{n-2}$, or the class of R is then reduced to

$$\overline{n^2 - n}\,\overline{n^2 - n - 1} - 9n^2 + 18n \quad \text{or} \quad n^4 - 2n^3 + n - 9n^2 + 18n = n(n^3 - 2n^2 - 9n + 19),$$

but this ought to be n; there is thus a further reduction of $n(n^3 - 2n^2 - 9n + 18)$, which is due to the double point of R. (? These are) * number $\frac{n}{2} \,.\, \overline{n-2}\,\overline{n^2-9}$ and (? so) * the double tangents of U are of this exact number.

Among mathematicians we can recall but Leibniz and Kirkman who have paid serious attention to mnemonics. The latter was greatly struck by the merits of the system set forth by Richard Grey, the friend of Dr. Johnson, in his *Memoria Technica* (1730), of which various editions appeared up to 1861. Kirkman thought but little of Feinagle and Coglan, whose systems had a fleeting popularity, and declared such men to be worthy of chairs in the University of Laputa. "Their cumbrous inventions are about as fit to be compared for elegance and speed, with the ἔπεα πτερόεντα of Richard Grey, as a Dutchman's ox-wagon in Kaffirland with a nobleman's chariot in Middlesex." So he published his *First Mnemonical Lessons in Geometry, Algebra and Trigonometry* (John Weale, 1852). The key is self-explanatory:

* Illegible.

b	d	t	f	l	s	p	k	n	z
1	2	3	4	5	6	7	8	9	0
a	e	i	o	u	au	oi	ei	ou	y

Thus 1851 is akla, or beila, or akub. The number π = tafaloudsutuknoint = 3·141592653589793 ; $\sqrt{2}$ = 1·4142135623731, and is bobodatusdipta. The example cot A sin C + cos b cos C = cot a sin b, which becomes

cot A*ng* si C*ang* and co *b* co C*ang* are cot *a* si *b*,

is not in the book mentioned above, and is probably in the paper of 1851 in the *Proceedings of the Literary and Philosophical Society of Manchester*. Kirkman's "Uncle Penyngton" is one of those clergymen "of mature knowledge, recognised ability, and blameless character", in whose case all suspicion on the part of the pupil is "irrational, and manifests a want of the power of appreciating evidence", as Todhunter would say. His two pupils, Richard and Jane, are chips from the old block. Jane's reply to an explanation of the mysterious $\sqrt{-1}$ by her revered preceptor is what one would expect: "So then whatever mystery or appearance of contradiction there may be here, it springs not from the answer of the oracle, but from the ignorance of the interrogator. His duty is not to cavil at the response, but to go away ashamed of himself and wondering". Uncle Pen. shared the prejudices of his age, for he says of mathematics, that it is a study in which, "My dear Jane . . . I should be sorry to see you deeply engaged." Richard shares little of the individuality of his charming sister. This is the book which De Morgan called "the most curious crotchet I ever saw." After all, one may over-estimate the soothing effect of jingles such as

RoóP slĕb . s̆ . sléc . slá,
Is Síne Bang hálf cá,
Is CHáSHca, is Area.

And who, by the following, would be kept from tripping in expanding $(1+r)^n$?

Sŭtón (ŭn *r*)? wrīte fr8 ŏn *r* ;
Theŭ *r* to í you multiply
By (i n-bácks bў i fags) :
If *n* hăs dĕn . é,
Put *r* vī (ré) ; top dits wĕd e.

After this one is not altogether astonished to see: "You will now find no difficulty. . . ."

It is interesting to note that the famous problem of the fifteen school girls, published in the *Lady's and Gentleman's Diary* for 1850 (Query VI, p. 48, and 1851, p. 48), was but an off-shoot from a paper Kirkman printed in the second volume of the *Cambridge and Dublin Mathematical Journal*. When the *Diary* went the way of all such things, the *Educational Times* stepped into the gap, and in the columns of the *Reprint* the name of Kirkman was for many years in evidence. We think he was the first to set his problems in (generally) amusing verse, and we seem to remember that in the long run Mr. Miller had to implore Kirkman's imitators to send their queries in plain prose.

Though sorely tempted we have not drawn upon the many good stories that are told in these lectures. A perusal of these pages will prove to the layman that mathematicians are not necessarily poor men of business, and that they are not as a rule devoid of humour.

One point in the lecture on Kirkman must not be passed over without echoing with Mr. G. B. Mathews his question to the responsible officials of the Royal Society. What has become of the unpublished part of Kirkman's monumental paper on the "Complete Theory of the Polyhedra", of which but

one-tenth has been printed? The subject is closely connected with group theory, and for a long time Kirkman was, if not the foremost, at least a very considerable figure among British contributors to the development of what was to prove " one of the most fundamental and fruitful notions in the whole range of our science ".

It was given to Macfarlane to lay these garlands on the tombs of men, most of whom he had known, and all of whom he honoured. His hearers little thought that the volumes containing his lectures would in their turn serve so soon to remind us of one who has left behind him memories, fragrant and appreciative, on both sides of the great ocean which divided us from his manifold activities. (W. J. GREENSTREET).

A Treatise on Probability. By JOHN MAYNARD KEYNES. Pp. xi + 466. 18s. net. 1921. (Macmillan.)*

This is undoubtedly the most important work on probability that has appeared for a very long time. Its importance is especially as regards the logical foundations of the subject, which are usually treated with extraordinary carelessness. I proposed first to give an abstract of Mr. Keynes's book, then to discuss briefly some of its more disputable portions.

Part I deals with " Fundamental Ideas ", and Chapter I deals with " The Meaning of Probability ". Mr. Keynes holds that a formal definition of probability is impossible, since he regards it as part of the fundamental apparatus of logic. Logic is accustomed to considering that relation between premiss and conclusion which enables us to infer the latter with certainty from the former, but this Mr. Keynes regards as only the extreme degree of the probability-relation, which subsists whenever one proposition has any bearing, favourable or unfavourable, upon the truth of another. Thus a proposition does not have a probability in itself, but only in relation to certain data. It may have different probability-relations to different sets of data, and all these are equally justified. Of course, when we want to use probability as a " guide to life ", in Bishop Butler's phrase, we must take account of all relevant knowledge ; but our estimate of probability relative to *that* knowledge will not be falsified by subsequent knowledge leading to a different estimate.

The probability of a proposition a relative to data h is represented by a/h. This is the fundamental symbol of the book. The inclusion of h in this symbol represents and entails much of Mr. Keynes's philosophy. It is remarkable how often the logic and philosophy of mathematical concepts has gone astray through the employment of symbols which did not contain explicitly all the variables upon which their value depended. If one were to employ such a symbol as (say) " $P(a)$ " for " the probability of a ", adding a proviso that this should be taken relatively to certain data h, it would be impossible practically to remember or express the relevance of h, and one's analysis would be certain to suffer sooner or later. The introduction of the symbol a/h is therefore of great importance. This symbol means the degree to which h makes a probable, or, as it may be expressed, the degree of belief which it is rational to give to a when h constitutes our relevant knowledge.

Mr. Keynes distinguishes between direct and indirect knowledge or rational belief, the latter being that part which rests upon argument. When the probability-relation between a and h is certainty ($a/h = 1$), we can *know* a when we know h ; this is indirect knowledge of a. But when the probability-relation is other than certainty, h does not enable us to know a, but only to attach a certain degree of rational belief to it. Probability, unlike certainty, does not enable us to *know* any proposition which omits mention of the premiss.

* Printed in the *Mathematical Gazette*, July, 1922.

Mr. Keynes maintains that probabilities are not always, even in theory, capable of numerical measurement. He maintains that it is not the case that of two unequal probabilities one must be the greater. To use an illustration (not Mr. Keynes's), the order of probabilities is like that of points on meridians of longitude: all lie between the North Pole (certainty) and the South Pole (impossibility), but points on different meridians do not lie on the same series. One of the meridians (that of Greenwich no doubt) represents numerically measurable probabilities; all the others represent probabilities which have no numerical measure. Mr. Keynes compares probabilities with similarities: there are degrees of similarity, but we cannot say that one similarity is always greater than, less than, or equal to, another. "For instance, a book bound in blue morocco is more like a book bound in red morocco than if it were bound in blue calf; and a book bound in red calf is more like the book in red morocco than if it were in blue calf. But there may be no comparison between the degree of similarity which exists between books bound in red morocco and blue morocco, and that which exists between books bound in red morocco and red calf." The chapter which discusses this subject abounds in good illustrations. For example, there is the lady to whom a jury awarded £100 for having been baulked of her chance of winning a prize in a beauty competition. Mr. Keynes holds that she got too much; evidently he thinks it *probable*, on the data, that she will not read his book.

The cases in which numerical measurement of probabilities is possible arise where there are a set of exclusive and exhaustive alternatives which are all equally probable on the data. This requires some principle by which we can judge directly, in suitable cases, that two probabilities are equal. The accepted principle is that of *non-sufficient reason*, or, as Mr. Keynes prefers to call it, of *indifference*. In its traditional form, this principle "asserts that if there is no *known* reason for predicating of our subject one rather than another of several alternatives, then relatively to such knowledge the assertions of each of these alternatives have an *equal* probability". This principle obviously requires limitation. *E.g.* if A is an object about which we have no information bearing on the question whether it is red or whether it is a book, the principle allows us to argue that the probability of "A is red" is $\frac{1}{2}$, and so is the probability of "A is a red book", whence it follows that if A is red it is a book. Mr. Keynes concludes that the principle in the above form is a necessary but not a sufficient condition of equiprobability. In order to arrive at a sufficient condition, it is first necessary to define *irrelevance*. It would be natural to say: h_1 is irrelevant to x on evidence h if $x/hh_1 = x/h$, *i.e.* if the addition of h_1 to our data makes no difference to the probability of x. But h_1 may consist of two parts, one of which increases the probability of x while the other diminishes it. To exclude this possibility, we define: h_1 is *irrelevant* to x/h if there is no proposition h_1', inferrible from h_1h but not from h, such that $x/h_1'h \neq x/h$. A proposition is defined as *relevant* when it is not irrelevant.*

By means of this definition, Mr. Keynes arrives at a more satisfactory enunciation of the principle of indifference: "There must be no *relevant* evidence relating to one alternative, unless there is *corresponding* evidence relating to the other; our relevant evidence, that is to say, must be symmetrical with regard to the alternatives, and must be applicable to each in the same manner." When this condition is fulfilled, the alternatives are

* I do not know whether Mr. Keynes has considered and rejected a definition of irrelevance which, *prima facie*, would be simpler than his. He does not state definitely whether *every* pair of propositions has some probability-relation, but I think he does not hold this view. I think he would say, *e.g.* that there is no probability-relation between the propositions "$2+2=4$" and "Napoleon disliked poodles". If so, it would seem natural to define h as irrelevant to a when a/h does not exist.

equally probable on the data. The discussion affects one of the stock elementary problems of probability. Given an urn containing white and black balls in unknown proportions, are we to suppose each ratio of white to black equally probable *à priori*, or are we to suppose that each ball is equally likely *à priori* to be white or black? The former is the commoner view, but the latter is alone correct.

There is need, however, of one further condition before the principle of indifference becomes applicable. The alternatives must be "indivisible", *i.e.* neither must be "capable of being further split up into a pair of possible but incompatible alternatives of the same form as the original pair". (For the formal definition, see p. 60.)

The principle of indifference allows, where it is applicable, numerical measurement of probabilities; for if there are n equally probable alternatives which are exhaustive and exclusive, the probability of each is $\frac{1}{n}$. But comparison of greater and less are possible in many cases where numerical measurement is impossible. We can compare the probabilities ab/h and a/h, and also the probabilities a/hh_1 and a/h. In the first case we keep the data constant and increase the conclusion; in the second, we keep the conclusion constant and increase the data. By means of these, many other comparisons become indirectly possible.

Part I concludes with two chapters, the first historical, the second dealing with the Frequency Theory advocated by Venn and many other writers. I shall return to this theory when I come to discuss Mr. Keynes's fundamental theses.

Part II, "Fundamental Theorems", gives the definitions and axioms upon which the formal reasoning of the rest of the book is based, together with some propositions readily derivable from the definitions and axioms. It is impossible to summarise this Part, since it is already as condensed as possible. I cite only, as essential to the whole formal structure, the definitions of addition and multiplication:

Addition: $ab/h + a\bar{b}/h = a/h$ (where $\bar{b}$ stands for not $-b$).

Multiplication: $ab/h = a/bh \,.\, b/h = b/ah \,.\, a/h$.

Thus addition and multiplication are not defined for any pair of probabilities, but only for such as have certain forms. This, of course, is connected with Mr. Keynes's view that probabilities are in general non-numerical. It is surprising how successful he is in raising his mathematical superstructure upon foundations which might have been thought inadequate to support it.

Part III, "Induction and Analogy", is the most important in the book from the point of view of philosophy and theory of knowledge. It has long since been proclaimed from the house-tops that all scientific knowledge rests upon induction, and it has been fairly evident that conclusions reached by induction were not certain, but at best more or less *probable*. Hume rejected induction as merely a fallacious habit; Mill tried to rehabilitate it. But it was soon seen that Mill's theories were mere eyewash, yet no better theories were invented to take their place. The philosophers had nothing sensible to offer to remedy the scandal. The writers on probability, on the other hand, offered too much: there was Laplace's theorem, according to which, when an event has happened m times, and failed n times, the chance that it will happen next time is $\frac{m+1}{m+n+2}$. This served admirably for proving that the sun will probably rise to-morrow. But it also proved that, if you ask the first man you meet in a village whether his name is Ebenezer Stick-in-the-mud, it is even odds that he will reply in the affirmative; and if he does, it is two to one that the next man you meet will have the same name. Mr. Keynes makes

short work of this theorem, which depends (among other errors) upon a wrong formulation of the principle of indifference. What he contributes is (as he admits) not yet a full and complete logic of induction, but I think it must form a part of the complete theory, and I do not think one can say as much for anything previously written on the subject.

Mr. Keynes maintains that Analogy is more fundamental than induction from mere number of instances. One may say that the purpose of inductive arguments is to prove, by examination of instances, that two characteristics A and B which are associated in those instances, are *probably* always associated. The object in practice is to make the probability as great as possible, and the object of theory is to show how to increase it and to discover circumstances (if there are such) under which it approaches certainty as a limit.

Given a set of instances all of which have the characteristics A and B, Mr. Keynes gives the name "positive analogy" to those characteristics which belong to all the instances, and the name "negative analogy" to those characteristics which belong to some but not all of the instances. None of these latter are invariably associated with B, therefore A's chance of being invariably associated with B is improved whenever the negative analogy is increased, just as one's chance of winning in a lottery is improved whenever some other competitor is found to have not drawn a prize. This assumes that there are prizes, *i.e.* that there are correlations. Mr. Keynes considers some assumption of this kind essential to the validity of the inductive method; this is a point to which we shall return later. He also holds that the only importance of numerous instances in induction lies in the probability that they may increase the negative analogy, since they may differ in some respect even if they are not known to do so. He gives the name "Pure Induction" to the argument from number of instances alone, where it is not *known* that they increase the negative analogy. The chief value of his discussion of Pure Induction, he says, "is negative and consists in showing that a line of advance, which might have seemed promising, turns out to be a blind alley, and that we are thrown back on known Analogy". But his discussion is so interesting that it must be reproduced in outline.

Let h represent the *à priori* data, g the generalisation we wish to establish; and $x_1, x_2, x_3, \ldots x_n$ instances of g. Let p_n represent the probability of the generalisation when $x_1, x_2, \ldots x_n$ have been observed, *i.e.*

$$p_n = g/hx_1x_2 \ldots x_n,$$

and let y_{n+1} represent the probability in the same circumstances, that the next instance will verify the generalisation, *i.e.*

$$y_{n+1} = x_{n+1}/hx_1x_2 \ldots x_n.$$

Put $p_0 = g/h =$ the *à priori* probability of the generalisation. Then we find

$$p_n = \frac{1}{y_1y_2 \ldots y_n} \cdot p_0,$$

which leads to

$$p_n = \frac{p_0}{p_0 + x_1x_2 \ldots x_n/gh\,(1 - p_0)},$$

where g stands for the falsehood of g. If we are to be able to have confidence in a pure induction when the instances are numerous, it is necessary that p_n should have 1 for its limit as n increases. This requires that $x_1x_2 \ldots x_n/\bar{g}h \cdot \dfrac{1}{p_0}$ should approach 0 as a limit as n increases. This will be the case if, for a specified s, p_s exceeds 0 by a finite amount (*i.e.* is equal to or greater than some finite *numerical* probability), and, whenever $r > s$, $x_r/x_1x_2 \ldots x_{r-1}gh$ falls short of certainty by a finite amount.

" In other words Pure Induction can be usefully employed to strengthen an argument if, after a certain number of instances have been examined, we have, from some other source, a finite probability in favour of the generalisation, and, assuming the generalisation is false, a finite uncertainty as to its conclusion being satisfied by the next hitherto unexamined instance which satisfies its premiss."

It might be thought that these conditions could be often fulfilled. But Mr. Keynes argues (and I do not see how to refute him) that we can have no assurance that p_0 (or p_s) exceeds some finite numerical probability, unless we bring in some new principle to confer a finite degree of probability on untested generalisations. Some such principle must therefore be sought.

Traditionally, the law of causation and the law of uniformity of nature were supposed to fulfil the required rôle. Mr. Keynes does not find much use for either of these. He interprets the law of the uniformity of nature as meaning that position in time and space are irrelevant to a generalisation. and asks in a footnote : " Is this interpretation of the Principle of the Uniformity of Nature affected by the Doctrine of Relativity? " Although the point is not perhaps very important in relation to the theory of induction, it is perhaps worth while to observe that the principle *is* vitally affected by relativity, being reduced from a law of nature to a tautology, since any two distinct events *must* differ otherwise than in absolute position, if there is no such thing as absolute position. Mr. Keynes proceeds :

" The kind of fundamental assumption about the character of material laws, on which scientists appear commonly to act, seems to me much less simple than the bare principle of Uniformity. They appear to assume something much more like what mathematicians call the principle of the superposition of small effects, or, as I prefer to call it, in this connection, the *atomic* character of natural law. The system of the material universe must consist, if this kind of assumption is warranted, of bodies which we may term (without any implication as to their size being conveyed thereby) *legal atoms*, such that each of them exercises its own separate, independent, and invariable effect, a change of the total state being compounded of a number of separate changes each of which is solely due to a separate portion of the preceding state."

This assumption does not yet afford a justification of induction, but it is not very different from the kind of assumption which Mr. Keynes finds necessary. He introduces what he calls the " independent variety " of a system of facts or propositions, which consists of the logical minimum by means of which everything else in the system can be defined or demonstrated. When the number of premisses constituting the independent variety is finite, the system is defined as finite. (Thus *e.g.* pure mathematics is a finite system.) Mr. Keynes proves that " if the premisses of our argument permit us to assume that the facts or propositions, with which the argument is concerned, belong to a *finite* system, then probable knowledge can be validly obtained by means of an inductive argument ". He suggests that, in any scientific inquiry which uses inductive methods, our procedure is not logically justifiable unless we assume (or have some reason to suppose) that we are dealing with a finite system in the above sense. A finite probability that we have to do with such a system suffices. But unless we can believe that there is such a finite probability, Mr. Keynes offers us no justification for the habit of employing inductive arguments.

The question of induction and analogy is so important that it seems desirable to set forth Mr. Keynes's views in somewhat greater detail. First, he reduces induction to analogy ; then he finds that

" As a logical foundation for Analogy, therefore, we seem to need some such assumption as that the amount of variety in the universe is limited in such a

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way that there is no one object so complex that its qualities fall into an infinite number of independent groups (*i.e.* groups which might exist independently as well as in conjunction) ; or rather that none of the objects about which we generalise are as complex as this ; or at least that, though some objects may be infinitely complex, we sometimes have a finite probability that an object about which we seek to generalise is not infinitely complex."

This is a sufficient, not a necessary, condition for the validity of analogical arguments. It is possible to weaken the condition considerably. It has often been maintained (I have done so myself) that the inductive principle, whatever it may be, cannot be proved by induction, since to do so would be circular. Mr. Keynes shows that this statement is more or less misleading.

" If our conclusion is C and our empirical evidence is E, then, in order to justify inductive methods, our premisses must include, in addition to E, a general hypothesis H such that C/H, the *à priori* probability of our conclusion, has a finite value. The effect of E is to increase the probability of C above the initial *à priori* value. . . . But the method of strengthening C/H by the addition of evidence E is valid quite apart from the particular content of H. If therefore we have another general hypothesis H' and other evidence E', such that H/H' has a finite value, we can, without being guilty of a circular argument, use evidence E' by the same method as before to strengthen the probability H/H'. . . .

" Our assumption, in its most limited form, then, amounts to this, that we have a finite *à priori* probability in favour of the Inductive Hypothesis as to there being some limitation of independent variety . . . in the objects of our generalisation. Our experience might have been such as to diminish this probability *à posteriori*. It has, in fact, been such as to increase it. It is because there has been so much repetition and uniformity in our experience that we place great confidence in it. To this extent the popular opinion that Induction depends upon experience for its validity is justified, and does not involve a circular argument."

It seems (though Mr. Keynes does not make this clear) that even in the above restricted form we still have a sufficient condition which has not been shown to be necessary. There may be other hypotheses besides that of finite variety which would justify the inductive method. I have no such hypothesis to suggest, but so far as I can see Mr. Keynes does not prove that none could be found. The possibility of other hypotheses would of course strengthen the case for the validity of induction. Mr. Keynes does not profess to have proved the validity of induction, but only to have discovered a hypothesis which, if it is true, will justify induction. This, I think, he has done. I think, also, that his hypothesis could be deduced from the theory of quanta. But that theory, of course, could not be established without the use of induction.

It is interesting to observe that, if Mr. Keynes is right, the validity of induction as a method of establishing the probability of generalisations depends upon a characteristic of the world which is not logically necessary. The world might be so constructed that induction would always lead us astray, not in the sense that improbable things would happen, but in the sense that the falsehood of inductive generalisations would not be improbable. Mr. Keynes thinks we do not live in such a world ; I hope he is right.

I have not space to deal with Part IV, " Some Philosophical Applications of Probability," or competence to appraise Part V, " The Foundations of Statistical Inference." The latter contains much admirable criticism. I wish it had contained some discussion of the statistical parts of physics, such as the second law of thermodynamics. In essence, statistical methods are an extension of induction, and raise no new question of principle. This Mr. Keynes brings out clearly.

Considering the book as a contribution to logic, three points seem specially important: (1) the view that probability is indefinable; (2) the contention that probabilities are not always (or even usually) numerically measurable; (3) the analysis of induction and analogy. Assuming the correctness of Mr. Keynes's views of the first two points, I do not think what he says on the third can be questioned, though it probably can be supplemented by further work founded upon it. But if he were mistaken on the first two points it might be possible to simplify his theory of induction. His difficulties here arise from the fact that the *à priori* probability of a generalisation has to be *finite*, *i.e.* greater than some numerically measurable probability other than zero. If all probabilities were numerically measurable, this condition would always be fulfilled. I shall not, therefore, further discuss induction, but shall briefly consider the other two points.

If probability is definable, the theory of probability will not be an independent part of logic, but (according to the only definitions that seem plausible) a branch of pure mathematics wholly derivable from the same apparatus from which the rest is derived. The only serious attempt, hitherto, to define probability is the frequency theory. Reduced to its simplest form, this theory states that, given two properties A and B, when we say that the chance that a thing which has the property A has the property B is p, we mean that p is the ratio of the number of things having both the properties A and B to the number of things having the property A. This theory is seen at its best in relation to such a question as: what is the chance that a number less than 100, chosen at random, will be a prime? Mr. Keynes has difficulties with such questions, because pure logic (which must be included in our premisses) enables us to prove that any actual number either is a prime or is not a prime, so that probability only exists before the number is specified. Nevertheless I am satisfied that his arguments against the frequency theory in its crude form are valid. He is less convincing in arguing against a modified form of the theory suggested to him by Dr. Whitehead.* His objections here are not addressed to the principle, but consist in showing technical difficulties which, one feels, might be overcome by ingenuity and skill. I think, at any rate, that there are very strong arguments for the view that probability attaches to a propositional function rather than to a proposition. Thus, in the above instance, the propositional function "x is a prime" has a probability-relation to the functional premiss "x is a number less than 100", but "4 is a prime" has not the same probability-relation to "4 is a number less than 100". It may be doubted how far this takes us in the direction of the frequency-theory; but it certainly involves if true, a not inconsiderable modification of Mr. Keynes's view.

The question whether probability is definable has of course an important bearing on the question whether probabilities are numerically measurable. On the frequency theory, probabilities *are* fractions. Some other definition may be found, which would enable us to prove either that probabilities are, or that they are not, numerically measurable. But, even if we accept Mr. Keynes's view as to the nature of probability, it may be doubted whether it follows that numerical measurement is sometimes theoretically impossible. Measurement is always largely conventional, and some convention might be invented for comparing probabilities lying on different paths between impossibility and certainty. There is also the following point: Mr. Keynes rejects the view that the probability of a proposition on given premisses can

* I take this opportunity to protest against Mr. Keynes's practice of alluding to *Principia Mathematica* as though I were the sole author. Dr. Whitehead had an equal share in the work, and there is hardly a page in the three volumes which can be attributed to either of us singly.

always be compared, as to greater and less, with that of its contradictory; if such comparison were always theoretically possible, every probability would be greater than, equal to, or less than $\frac{1}{2}$. If, then, p and q were two independent propositions each of which was less likely than its contradictory, the probability of pq would be less than $\frac{1}{4}$. In this way, an indefinite number of numerical approximations would become possible. I do not know whether it can be maintained that the probability of a proposition is always comparable with that of its contradictory, but it seems evident that, where this is not the case, it is not rational to allow the probability of the proposition to influence conduct. In the chapter on " The Application of Probability to Conduct ", Mr. Keynes seems to suggest, though he does not definitely assert, that non-numerical probabilities may rationally influence conduct; and elsewhere he suggests that it is rational to give weight to an uncertain induction when no better induction can be ascertained. I cannot see how this can be justified except when the probability of the proposition in question is known (or believed likely) to be greater than that of its contradictory. It seems to me, therefore, by no means certain that probabilities are incapable of numerical measurement (at least approximately), except in the very special cases allowed by Mr. Keynes's use of the principle of indifference. These are, however, only doubts; I have nothing positive to urge as against Mr. Keynes's scepticism.

Much of the criticism in the book is extremely valuable, and the mathematical calculus is astonishingly powerful considering the very restricted premisses which form its foundation. The book as a whole is one which it is impossible to praise too highly, and it is to be hoped that it will stimulate further work on a most important subject which philosophers and logicians have unduly neglected.

BERTRAND RUSSELL.

1564. (145). DIRICHLET'S " BOXES " IN THE 17TH CENTURY. Nicole, one of the authors of the famous *Logic of Port-Royal*, relates the following: " One day I told Madame de Longueville that I could prove that there are at least two people living in Paris with the same number of hairs on their heads. She asserted that I could never prove this without counting them first. My premisses are these. No head has more than 200,000 hairs, and the worst provided has one. Consider 200,000 heads, none having the same number of hairs. Then each must have a number of hairs equal to some number from 1 to 200,000 both included. Of course if any have the same number of hairs my bet is won. Now take one more person, who has not more than 200,000 hairs on his head. His number must be one of the numbers 1 to 200,000 included. As the inhabitants of Paris are nearer 800,000 than 200,000, there are many heads with an equal number of hairs."

1565. (167). Many years ago the late Dr. Hayward invited the Council of the A.I.G.T. to meet at Harrow, giving generous hospitality. At lunch the conversation turned on Euclid's test of proportionals, and our host defended the system of multiples, and added, " We should have a good idea of the relative contents of this wine glass and that tumbler if we counted the number of times they went, respectively, into a barrel ". Whereupon Dr. Sophie Bryant replied, " Wouldn't it do equally well if we counted the number of times a thimbleful of water would go into each? " [Per Professor R. W. Genese.]

THE A.I.G.T. AND THE M.A.

SOME NOTES BY A. W. SIDDONS.

ANYONE who wades through my Presidential Address of January, 1936 (*Mathematical Gazette*, Vol. XX, No. 237), will find an account of the work of the A.I.G.T. and the M.A., but I think it may be of interest to see some of the landmarks of that work put out in a more accessible form. The following facts are collected mainly from the Annual General Reports of the A.I.G.T. and the Mathematical Association.

Membership of the A.I.G.T. It started in 1871 with 61 members, grew to 100 by 1873 and crept up to 150 by 1886. From 1888 till 1896 the numbers were generally between 180 and 190. In 1897 the A.I.G.T. became the M.A.

Membership of the M.A. Between 1898 and 1913 the number of members rose fairly steadily from 229 to 700. From 1913 to 1921 it hovered between 650 and 760. From 1922 to 1940 it rose steadily from 832 to 1784. During the war the numbers dropped a little; in 1946 the number was 1772 and in 1947 it was 2312.

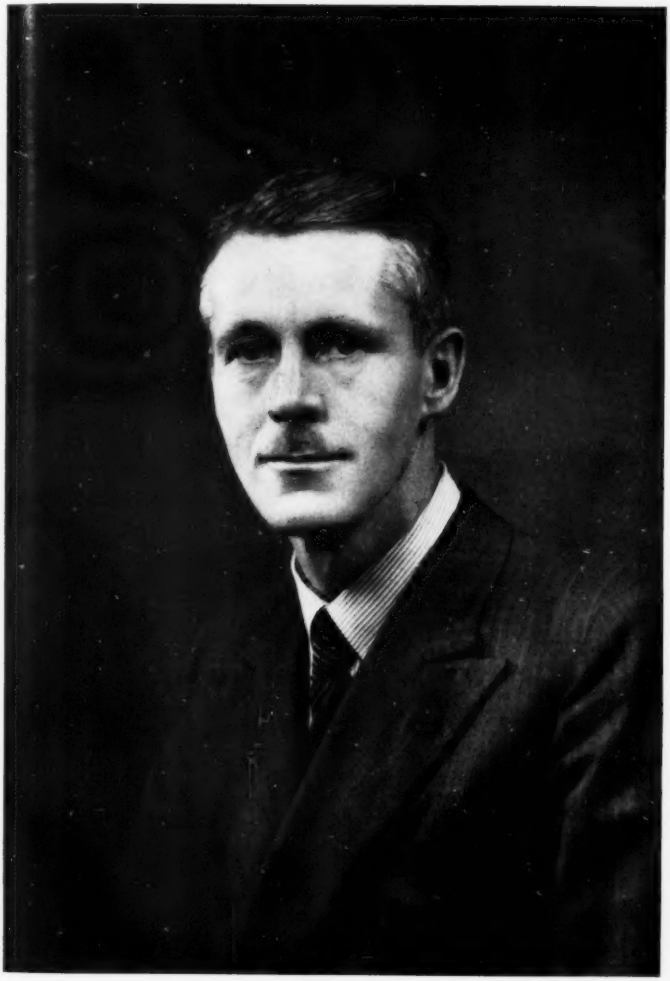
Attendance at meetings. The first annual meeting of the A.I.G.T. was attended by 26 members and a few visitors; the second meeting by 38 members and some visitors. After that until 1893 the attendance varied between 20 and 30. I have found no records after that.

My recollection of the M.A. meetings between 1900 and 1902 is that the attendance was not more than 30. By 1903 the Teaching Committee had published reports on Geometry, Algebra and Arithmetic; these were discussed at the annual general meeting of 1903 and from that time onwards the attendance at meetings grew steadily until now it runs into hundreds.

It is of interest to note that the first meeting of the London Branch was attended by about 220.

THE A.I.G.T.

1870. In *Nature*, 26th May, a letter from R. Levett proposed the formation of an Anti-Euclid Association. In October, a list of 28 members of "The Association for the Reform of Geometrical Teaching" was circulated and a meeting called for January 1871.
1871. On 17th January, a meeting was held at University College, London. It was decided to adopt the name "Association for the Improvement of Geometrical Teaching".
1872. Decided to produce a *Syllabus of Plane Geometry*.
1875. Decided to publish the syllabus.
1878. Sub-committees appointed to consider Solid Geometry, Higher Plane Geometry, Geometrical Conics.
1881. Decided to produce a *Textbook of Elementary Plane Geometry*.
1882. Proofs of Book I circulated to members.
1883. Proofs of Book II circulated to members. The afternoon session of the annual meeting was devoted to three papers on Mechanics. Sub-committee appointed to consider Arithmetic. Books I and II of the *Textbook* were published in October.
1884. Sub-committee appointed to consider Mechanics.
1886. Books III, IV and V of the *Textbook* were published.
1887. *Syllabus of Geometrical Conics* published.
1887 and 1888. Oxford and Cambridge decided "to accept proofs other than Euclid's provided that they do not violate Euclid's order".
1889. *Syllabus of Modern Plane Geometry* published.
1890. *Syllabus of Elementary Dynamics, Part I: Linear Dynamics*, published.
1894. First number of the *Mathematical Gazette* published.



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THE M.A.

1897. In March the name " Mathematical Association " was adopted. It was agreed that General Meetings of the Association should be held in London at least once a term, and in other places if desired. Notice to this effect continued to appear on the cover of the *Mathematical Gazette* until 1913, but I can find no notices calling more than the one meeting a year after 1903, and I have no recollection of meetings other than the annual meeting being held since I became a member in 1899.

1902. The first Teaching Committee was appointed.

1903. Oxford and Cambridge decided not to be bound by Euclid's order, but to accept any proof that appeared to form part of a systematic treatment of the subject; and to accept proofs only applicable to commensurable magnitudes and proofs using hypothetical constructions.

1905. For the first time the Council's Report was printed in the *Mathematical Gazette*. These reports have been printed there regularly since that date.

BRANCHES OF THE M.A.

The dates given are the dates of formation. Most of these dates have been taken from the Reports of the Council; for a few I am indebted to Branch Secretaries.

1907 North Wales	1928 North-Eastern
1909 London	1929 Liverpool
1909 Southampton *	1933 South-West Wales
1920 Yorkshire	1935 North Ireland
1920 Bristol	1939 Sheffield
1920 Manchester	1939 Plymouth
1923 Cardiff	1939 Nottingham
1926 Midland	
1911 Sydney, N.S.W.	1924 Victoria
1922 Queensland	1935 Auckland, N.Z.

In 1939 some Junior Branches were formed.

PRESIDENTS OF THE A.I.G.T.

1871 Prof. T. A. Hirst	1892 C. Taylor
1878 R. B. Hayward	1893 R. Wormell
1889 Prof. G. M. Minchin	1895 J. Larmor
1891 Prof. J. J. Sylvester	

PRESIDENTS OF THE M.A.

1897 Prof. A. Lodge	1926 Prof. M. J. M. Hill
1899 Sir Robert S. Ball	1928 W. F. Sheppard
1901 J. Fletcher Moulton	1930 Prof. A. S. Eddington
1903 Prof. A. R. Forsyth	1932 Prof. G. N. Watson
1905 G. B. Mathews	1934 Prof. E. H. Neville
1907 G. H. Bryan	1935 A. W. Siddons
1909 Prof. H. H. Turner	1936 Prof. A. R. Forsyth
1911 Prof. E. W. Hobson	1937 Prof. L. N. G. Filon
1913 Sir Geo. Greenhill	1938 W. Hope-Jones
1915 Prof. A. N. Whitehead	1939 W. C. Fletcher
1917 Prof. T. P. Nunn	1944 C. O. Tuckey
1919 Prof. E. T. Whittaker	1945 Prof. S. Chapman
1921 Canon J. M. Wilson	1946 W. F. Bushell
1922 Sir Thomas Heath	1947 Prof. G. B. Jeffery
1924 Prof. G. H. Hardy	1948 Sir Harold Spencer Jones

* This Branch lapsed in 1914, but was re-started again in 1931.

SECRETARIES OF THE A.I.G.T.

1871 R. Levett, E. F. M. MacCarthy
1878 R. Levett, R. Tucker
1883 R. Levett, E. B. Sargant
1884 J. B. Lock, E. B. Sargant
1885 E. M. Langley, E. B. Sargant
1886 E. M. Langley, C. Pendlebury
1894 R. Holmes, C. Pendlebury
1896 C. Pendlebury, J. J. Milne

SECRETARIES OF THE M.A.

1897 C. Pendlebury, S. O. Roberts
1899 C. Pendlebury
1900 C. Pendlebury, H. D. Ellis
1908 C. Pendlebury, H. D. Ellis, Miss E. Greene
1910 C. Pendlebury, H. D. Ellis, Miss E. R. Gwatkin
1912 C. Pendlebury, H. D. Ellis, Miss M. Punnett
1916 C. Pendlebury, Miss M. Punnett
1936 G. L. Parsons, Miss M. Punnett
1939 G. L. Parsons, Mrs. E. M. Williams
1946 G. L. Parsons, Miss M. E. Bowman

EDITORS OF THE *Gazette*

Nos. 1–6. E. M. Langley
Nos. 7–15. F. S. Macaulay
Nos. 12–15. F. W. Hill
Nos. 16–208. W. J. Greenstreet
Nos. 209–210. E. H. Neville
Nos. 211– . T. A. A. Broadbent

A. W. S.

We venture to append to Mr. Siddons' lists a few notes pertaining specially to the *Gazette*.

No. 103 (1913 Jan.) was a special number, prepared in honour of E. M. Langley and W. J. Greenstreet by A. Lodge with the assistance of C. S. Jackson.

During the illness of W. J. Greenstreet, in 1927, several numbers were edited by E. H. Neville.

ASSISTANT EDITORS

Nos. 16–18; 1899 Feb.—1899 Oct. - F. S. Macaulay, H. W. Lloyd Tanner
Nos. 19–105; 1900 Feb.—1913 May - F. S. Macaulay, H. W. Lloyd Tanner, E. T. Whittaker
Nos. 106–210; 1913 July—1930 Dec. - F. S. Macaulay, E. T. Whittaker

PROBLEM EDITOR

Nos. 24–57; 1900 Dec.—1906 May - - W. E. Hartley

PRINTERS AND PUBLISHERS

Nos. 1–6 (the numbers in quarto) were printed by R. & R. Clark and published by Macmillan & Co. Nos. 7–300 were printed by MacLehose & Co. and published by Bell & Sons.

LARGE NUMBERS.

By J. E. Littlewood.

§1. The problem how to express very large numbers is discussed in 'The Sand-Reckoner' of Archimedes. Grains of sand being proverbially 'innumerable', Archimedes develops a scheme, the equivalent of a 10^n notation, in which the 'Universe', a sphere reaching to the Sun and calculated to have a diameter less than 10^{10} stadia, would contain, if filled with sand, fewer grains than '1000 units of the seventh order of numbers', which is 10^{51}. [A myriad-myriad is 10^8; this is taken as the base of what we should call exponents, and Archimedes contemplates 10^8 'periods', each containing 10^8 'orders' of numbers; the final number in the scheme is $10^8 \cdot 10^{15}$.] The problem of expression is bound up with the invention of a suitable *notation*; Archimedes does not have our a^b, with its potential extension to $a^{a^{a^{\cdot^{\cdot^{\cdot}}}}}$. We return to this question at the end; the subject is not exhausted.

§2. Certain ancient Indian writings reveal an awestruck obsession with ideas of immense stretches of time. See Buckle's *History of Civilisation in England*, pp. 121–124 (2nd edition). (I *thought* the following came from there; I cannot have invented it, surely).

There is a stone, a cubic mile in size, a million times harder than diamond. Every million years a very holy man visits it to give it the lightest possible touch. The stone is in the end worn away. This works out at something like 10^{35} years; poor value for so much trouble, and an instance of the 'debunking' of popular immensities.

§3. The Greeks made an enormous leap of the imagination in conceiving the heavenly bodies as objects dispersed in 'ordinary' space. A similar if lesser leap was needed to initiate the now familiar geological arguments about erosion and the like. It is easy to imagine Archimedes doing this, but so far as I know no Greek did. It can be a mildly entertaining exercise to check, for example, the scooping out of a valley by a trickle of stream, unthinkable to common sense. A twentieth of an inch a year is a mile in 10^6 years; this, if continued, would be a thousand miles in 10^9 years. (These times are natural units; the second is comparable with the age of the Earth, the first is the time it takes to turn an ape into a Ph.D.)

Newton estimated the distance of Sirius (in astronomical units), assuming it to be comparable with the Sun. His method was to compare Sirius and Saturn, guessing (correctly) the albedo of the latter.

§4. The next two items complete my references to the past. The first is the accuracy of Tycho Brahe's angular measurements. These were correct to 1′, which I find surprising (Hipparchus's to 4′). The other is the Samos tunnel, described in Farrington's very interesting Pelican book, *Greek Science*, p. 37. Made at about the time of Pythagoras, it was 900 yards long and was begun at opposite ends; the junction in the middle is shown by modern digging to have been within a couple of feet. I am not concerned with the mild ideological axe-grinding of the book, but it is surely simplest to disbelieve that this was an achievement of surveying technique. The relevant *principles* of similar triangles existed since Thales, but I find the instrumental accuracy incredible. I can on the other hand easily believe in a line of posts over the hill, or at a pinch in sighting on a star from opposite sides.

§5. I come to modern times, but continue the topic of measurements.

We all know that measured parallaxes deal in quantities of the order of 0″·001 (average error 0″·025); does every reader realise that this is the angle subtended by a penny 4000 miles away?

Astronomical measurement of time provides the greatest number of significant figures used in scientific calculation; measurement is to ·001″, it is possible to deal in 10^3 or 10^3 years, and with a couple of extra figures for purposes of calculation we have a total of 15.

I once asked Eddington what accuracy was possible in measuring the angular separation of widely separated stars. To the outsider the mechanical difficulties seem enormous, as do those of dealing with refraction; the answer (given instantly) was 0″·1, which I for one found very surprising.

I will hand on another surprise. The principles of an Ordnance Survey clearly involve, first, something of the nature of a 'rod', which is placed in successive positions until we arrive, secondly, at a 'base', from which we carry on by angular measurements. The questions are: what are the most efficient lengths of rod and base? The 'rod' is a metal ribbon 130 inches long, which is much what anyone would expect; but the length of the base, which is 9 miles, is about 100 times what I should have guessed. In such matters, of course, the really determining difficulty is apt to be something not obvious and interesting, but unexpected and dull: apparently the trouble is that of placing a theodolite accurately over the right spot.

§6. We shall presently be considering multiple exponentials and we must consider the principles of their 'approximate' nature. Since 'order' has a technical meaning not suited to our purposes, we will speak of 'types' of numbers,

$$N_1 = 10^{10}, \quad N_2 = 10^{10^{10}}, \ldots, \quad N_n = 10^{N_{n-1}},$$

which we describe as of type 1, 2, ..., n, ... We further describe

$$10^{10^{10^{4\cdot7}}},$$

for example, as of type 2·47, and write it $N_{2\cdot47}$. This makes the point that its type is between 2 and 3; there is a slight inaccuracy in that it is $N_{2\cdot1}$ and not $N_{2\cdot0}$ that is N_2—we may ignore this. We also call it $N_2(4\cdot7)$ when we wish to express its mathematical form in brief notation: note that N_n is $N_n(1)$. The number 10^{79}, which (with apologies for the small letter) we will call u, as being the number of ultimate particles in the Universe, is $N_{1\cdot19}$.

The principle I now wish to establish is sufficiently illustrated by the following instances. A number of type 2 or over is 'practically unaltered' by being squared; a number of type 3 or over is what we may fairly call 'unaltered' by being raised even to the power u. In fact, while

$$N_2 = 10^{10^{10}},$$

we have

$$N_2^{\,2} = 10^{10^{10\cdot3}};$$

and while

$$N_3 = N_3(1),$$

we have

$$N_3^{\,u} = N_3(1 + 7\cdot9 \,.\, 10^{-9}).$$

Again, N_2 is hardly altered by having its bottom 10 changed into u, and is 'unaltered' by having it changed into 2. Another constantly relevant point is that for an N of type 1 or over there is 'no difference' between $N!$ (or N^N) and 2^N.

We may sum up these considerations as the 'principle of crudity': the practical upshot is that in estimating a number $a^{b^{c^{\cdot^{\cdot}}}}$ it is worth taking trouble to whittle down the top index, but we can be as crude as we like about things that bear only on the lowest ones.

§7. I come now to numbers directly connected with daily life (what I mean by 'indirectly' will appear in §11). The range from just perceptible to just

tolerable sound (at the same pitch, and where sensitivity is maximal) is over 10^{12}. In the case of light the range is (as we should expect) even greater. The surface of the sun has $6 . 10^5$ times the brightness of the full moon (incidentally the sun is $5 . 10^6$ times as bright as the *half* moon). A sandy surface lit by the full moon is accordingly in a similar relation with the surface of the full moon. Anyone who has walked on a country road on a moonless night with heavy cloud knows that one can still perceive the road or objects on it (I am not myself satisfied that anyone has properly explained where the light comes from); there must be a new factor of at least 10^3 (I should say 10^4 or more, the total being 10^{14} or 10^{15}).

At one time it was possible to buy 10^{13} ergs for 4d.; nothing about energy of mass, merely the British Heat Unit: the erg is ,of course, absurdly small, and the mechanical equivalent of heat very large.

Coincidences and Improbabilities.

§8. Improbabilities are apt to be overestimated. It is true that I should have been surprised in the past to learn that Professor Hardy had joined the Oxford Group. But one could not say the adverse chance was $10^6 : 1$. Mathematics is a dangerous profession; an appreciable proportion of us go mad, and then this particular event would be quite likely.

A popular newspaper noted during the 1947 cricket season that two batsmen had each scored 1111 runs for an average of 44·44. Since it compared this with the monkeys' typing of *Hamlet* (somewhat to the disadvantage of the latter) the event is worth debunking as an example of a common class (the same paper later gave a number of similar cases). We have, of course, to estimate the probability of the event happening at some time during the season. Take the 30 leading batsmen and select a pair A, B of them. At some moment A will have played 25 complete innings. The chance against his score then being 1111 is say $700 : 1$. The chance against B's having at that moment played 25 innings is say $10 : 1$, and the further chance that his score is 1111 is again $700 : 1$. There are, however, about $30 . 15$ pairs *; the total adverse chance is $10 . 700^2/(30 . 15)$, or about $10^4 : 1$. A degree of surprise is legitimate.

A report of holding 13 of a suit at Bridge used to be an annual event. The chance of this in a given deal is $2·4 . 10^{-9}$; if we suppose that $2 . 10^8$ people in England each play an average of 30 hands a week the probability is of the right order. I confess that I used to suppose that Bridge hands were not random, on account of inadequate shuffling: Borel's book on Bridge, however, shows that since the distribution within the four hands is irrelevant the usual procedure of shuffling is adequate. (There is a marked difference where games of Patience are concerned: to destroy all organisation far more shuffling is necessary than one would naturally suppose; I learned this from experience during a period of addiction, and have since compared notes with others.)

I sometimes ask the question: what is the most remarkable coincidence you have experienced, and is it, for *the* most remarkable one, remarkable? (With a lifetime to choose from, $10^6 : 1$ is a mere trifle.) This is, of course, a subject made for bores, but I own two, one startling at the moment but debunkable, the other genuinely remarkable. In the latter a girl was walking along Walton St. (London) to visit her sister, Florence Rose Dalton, in service at number 42. She passed number 40 and arrived at 42, where a Florence Rose Dalton was cook (but absent for a fortnight's holiday, deputised for by her sister). But the house was 42 Ovington Sq. (the exit of the Square narrows to road width), 42 Walton St. being the house next further on. (I was staying at the Ovington Sq. house and heard of the occurrence the same evening.)

* Note that it is pairs and not ordered pairs that are relevant.

In the other, 7 ships in Weymouth Harbour at the beginning of a 3 mile walk had become 6 when we sat down to rest : the 6 were riding parallel at their anchors, but the two-masted 7th had aligned itself exactly behind a mast of one of the 6. A shift of 5 yards clearly separated the masts. The chance against stopping in the right 10 yards is 600 : 1 ; that against the ship being end on about 60 : 1 ; in all about 4.10^4 : 1 ; the event is thus comparable to the cricket average both in striking impact and real insignificance.

There must exist a collection of well-authenticated coincidences, and I regret that I am not better acquainted with them. Dorothy Sayers in " Unpopular Opinions ", cites the case of two negroes, each named Will West, confined simultaneously in Leavenworth Penitentiary, U.S.A. (in 1903), and with the same Bertillon measurements. (Is this really credible?)

Eddington once told me that information about a new (newly visible, not necessarily unknown) comet was received by an Observatory in misprinted form ; they looked at the place indicated (no doubt sweeping a square degree or so), and saw a new comet. (Entertaining and striking as this is the adverse chance can hardly be put at more than a few times 10^6.)

§9. We all remember the schoolboy doodle of tracing a pencil line down a printed page through the spaces between words. Suppose we take a small-print encyclopedia with about 100 lines to the page, and slash a line through at random. With a 5 : 1 chance against succeeding at a given line the chance against performing this doodle is 10^{70}.

My next instance is perhaps off the main track. There is a certain procedure by which a conjuror may perform the apparently impossible. A card, say the Ace of Spades, being selected the conjuror places the pack on the table and asks the subject to think of a number less than 100. There is a very fair chance that he will select 37 ; in this case he is told to count down and take the 37th card (which is the Ace) : if another number is selected *the conjuror does some other trick*. (A milder form deals with numbers less than 10 ; the selection is very likely to be 7, and if not, then 3 ; with 9 cards, and the Ace 7th he succeeds outright in the first case and can proffer the inverted pack in the second.)

In my present category belongs the chance typing of *Hamlet* by the monkeys. With say 27000 letters and spaces to be typed and say 35 keys the adverse chance is $35^{27000} < N_{1\cdot5}$.

Games.

§10. Suppose that in a game of position there are p possible positions $P_1, P_2, \ldots, P_p$. A game is a finite sequence of P's, each derived from the preceding by a " move " in accordance with the rules. p is generally of type slightly greater than 1, and the number of games may consequently be comparable with $p!$ or 2^p, which brings us for the first time to a type above 2. The crudity principle will be in operation.

In Chess, a game is a draw if the same position occurs for the 3rd time in all. (As a matter of fact the game continues unless one of the players exercises the right to claim a draw ; to avoid the consequent infinity we will suppose there *is* a draw.) The rules do not say whether for this purpose the men (' man '= ' piece or pawn ') retain their identity ; we shall suppose that they do.*

What is the chance that a person A, ignorant of the rules, will defeat C, the world champion? Suppose that in practice C, in 1 out of n of his games, loses in not more than m moves. We suppose that A knows that when it is his turn

* I learn from Mr. H. A. Webb that in one of Blackburne's games a position recurred for a second time, but with a pair of rooks interchanged. Each player expected to win, otherwise (as Blackburne said) a delicate point for decision would have arisen.

to move he must place one of his men on an unoccupied square, or on an enemy-occupied square, with capture. When he has $N \leqslant 16$ men he has a choice of $N^{64-N} \leqslant M = 16^{48}$ actions. There is, in effect, once in n games, a sequence of m actions leading to victory; his chance in all is better than $1 : nM^m$. The number n hardly matters if we can reduce m; if we may suppose that m is 20 * for $n = 10^6$, A has a chance better than $10^{-123} = 1/N_{1\cdot 21}$ (more likely than two in succession of the doodles of §9.)

What is the number of possible games of Chess? It is easy to give an upper bound. A placing (legal or not) of men on the squares of the board we will call an 'arrangement', A, one possible in Chess we will call a 'position', P. A change from one A to another we call a 'shift', and a legal Chess move (from a P) a 'move'. With N men in all on the board there are (with the 'individuality' convention) $64!/(64-N)!$ A's. (As a matter of fact, since *all* pawns can be promoted, it is possible for something like this number to be actually P's; the only legal bars are that the K's must not be contiguous, nor both in check, and that if there are 10 white (or black) bishops, they cannot all be on squares of one colour). The number of sets of men (irrespective of their placing) composed of pawns and pawns promoted (to Q, R, B or Kt) is

$$5^{16} + 5^{15} + \ldots + 5^1 < 2 \,.\, 10^{11}.$$

Since the two K's must be present the number of sets of *pieces* other than those promoted from pawns is

$${}_{14}C_1 + {}_{14}C_2 + \ldots + {}_{14}C_{14} < 14 \,.\, {}_{14}C_7 < 5 \,.\, 10^4;$$

hence the number of sets of N men is for every N less than 10^{16}.

The number of moves possible from a P (or for that matter an A) is at most

$$\mu = 9 \,.\, 28 + 2 \,.\, 14 + 2 \,.\, 14 + 2 \,.\, 8 + 8 = 332.$$

The number of A is less than

$$a = 10^{16} \sum_{N=1}^{32} \frac{64!}{(64-N)!} = 10^{69\cdot 7}.$$

The number of possible games is at most

$$\mu^{2a+1} = 10^{10^{70\cdot 5}} = N_{2\cdot 185}.$$

The problem of a not too hopelessly inadequate lower bound (even a moral certainty without full proof) seems not at all easy. Unless there are a fair number of mobile men the number of positions, which dominates the top index, is inadequate; with a number of men, however, it is difficult to secure their *independent* ranging through long sequences of moves. We may consider Kings at corners protected from check by 3 minor pieces, and some Queens of each colour (9 if possible). I have thought of this question too late to try to develop a technique; perhaps some readers may compete for a record.

§11. We come now to the numbers that I describe as indirectly connected with daily life. These arise out of the enormous number $n_0 = 3 \,.\, 10^{19}$ of molecules per c.c. of gas under standard conditions, and the permutations connected with them. I will recall the admirable illustration of Jeans, that each time one of us draws a breath it is highly probable that it contains some of the molecules of the dying breath of Julius Caesar.

What is the probability that the *manuscript* (as opposed to a typescript) of *Hamlet* came into existence by chance; say the probability that each of the n molecules of the ink found its chance way from an ink-pot into *some*

* We may suppose that C (in the light of A's performance!) does not suspect the position, and resigns in the ordinary way.

point of an ink-line of script recognisable as the text of *Hamlet*? We can choose half the molecules in the actual ink-line to determine a narrow region into which the other $\frac{1}{2}n$ molecules have to find their chance way. If the chance for a single molecule is f the relevant chance is $f^{\frac{1}{2}n}$. Since n is of the order of $500n_0 = 1{\cdot}5\,.\,10^{21}$, the crudity principle operates, and it makes no difference whether f is 10^{-1} or 10^{-10}. The adverse chance is $N_{2\cdot13}$.

We believe, of course, that something happened which at first sight is much more improbable; the ink came into position in orderly succession in time. But what is the additional factor? If we call the ink of a fullstop a 'spot', the ink-line is made up of say $s = 10^6$ spots. We must multiply the original number by $s!$, but this leaves it quite unaffected. (Similarly the latitude of choice implied by the italicised '*some*' above makes no difference.)

§12. We all know that it is merely probable, not certain, that a kettle on a gas-ring will boil. Let us estimate the chance, by common consent small, that a celluloid mouse should survive for a week in Hell (or alternatively that a real mouse should freeze to death). Piety dictates that we should treat the problem as classical, and suppose that the molecules and densities are terrestrial. We must not belittle the Institution, and will suppose a temperature (absolute) of $T_H = 2{\cdot}8\,.\,10^{12}$ (the 2·8 is put in to simplify my arithmetic).

Let c be the velocity appropriate to temperature T, $T_0 = 280$ (English room temperature), $c_0 = c(T_0)$, $c_H = c(T_H)$. Let $\mu = kn_0$ be the number of molecules in the mouse ($k = 10^3$, say). The chance p that a given molecule has $c \leqslant c_0$ is, in the usual notation,

$$4\pi \int_0^{c_0} \left(\frac{hm}{\pi}\right)^{3/2} e^{-hmc^2} c^2\, dc.$$

This is of order (the constant is irrelevant by the crudity principle)

$$p = (c_0/c_H)^3 = (T_0/T_H)^{3/2}.$$

The chance that most of the mouse has $c \leqslant c_0$ is not much better than p^μ. Let τ be the 'time of relaxation' at temperature T; this is comparable with the time of describing a free path; then

$$\tau_H = \tau_0 c_0/c_H = \tau_0 (T_0/T_H)^{1/2},$$

and τ_0 is of order

$$\tau_0 = n_0^{-1/3}/c_0.$$

In a week there are $\nu = w/\tau_H$ time intervals of length τ_H, where w is the number of seconds in a week.

Now an abnormal state subsides in time of order τ_H, and a fresh 'miracle' is needed for survival over the next interval. The total adverse chance against survival for a week is therefore of type

$$C = (p^{-\mu})^\nu = \left(\sqrt{\frac{T_H}{T_0}}\right)^{\frac{3}{2} c_0 k w n_0^{4/3} \sqrt{\frac{T_H}{T_0}}}.$$

With numerical values

$$n_0 = 3\,.\,10^{19},\ k = 10^3,\ c_0 = 4\,.\,10^5,\ w = 5\,.\,10^5,\ \sqrt{(T_H/T_0)} = 10^5,$$

we have

$$C = 10^{10^{46\cdot1}} = N_{2\cdot17}.$$

Of the 46·1, 5 comes from T_H/T_0, 5·7 from w, 5·6 from c_0, and most from n_0.

Factorisations.

§13. The days are past when it was a surprise that a number could be proved prime, or again composite, by processes other than testing for factors up to the square root; most readers will have heard of Lehmer's electric sieve, and some at least will know of his developments of the 'converse of Fermat's

theorem ' by which the tests have been much advanced.* For comparison with our other numbers I will merely recall : $2^{127}-1 \sim 10^{38}$ is the greatest known prime ; $2^{257}-1 \sim 10^{76}$ is composite though no factor is known, and it holds the record in that field ; $2^{2^6}+1 \sim 10^{19}$ has factors

$$274177 \quad \text{and} \quad 67280421310721.$$

Most numbers that have been studied have naturally been of special forms like $a^n \pm b^n$; these are amenable to special tests of 'converse Fermat' type. I asked Professor Lehmer what size of number N, taken at random, could be factorised, or again have its prime or composite nature determined, within, say, a year, and (*a*) for certain, (*b*) with reasonable certainty, (*c*) with luck. Much depends on the nature of $N-1$. If a reasonably large factor or product of factors of this is known the 'converse Fermat' processes will decide the nature of N, and this for N with 50 to 100 digits. Similar results can be obtained if we can find factors of $N+1$. Generally both $N \pm 1$ will have many small factors. The disaster of finding all three of N, $N-1$, $N+1$ resisting factorisation must be exceedingly rare (and it suggests theoretical investigation). If, however, it occurs the value of $a^N - 1 \pmod N$, with, say, $a=2$, can be calculated. If this is different from 1 then N is of course composite ; if N *is* composite the test, if we may judge by smaller N (of order 10^{10}) is very likely though not certain to succeed. If the value is 1, N is (accordingly) very likely but not certain to be prime. In this final case there remains, for definite proof, none but 'direct' methods, and these are applicable only up to about 10^{20}.

Professor Lehmer further tells me that numbers up to $2{\cdot}7 \,.\, 10^9$ can be completely factorised in 40 minutes ; up to 10^{15} in a day ; up to 10^{20} in a week ; finally up to 10^{100}, with some luck, in a year.

$\pi(x) - \mathrm{li}(x)$ *and the Skewes number.*

§14. The difference $d(x) = \pi(x) - \mathrm{li}\, x$, where $\pi(x)$ is the number of primes less than or equal to x, and $\mathrm{li}\, x$ is the (principal value) logarithmic integral $\int_0^x \frac{dx}{\log x}$, is negative for all x up to 10^7 and for all x for which $\pi(x)$ has been calculated. I proved in 1914 that there must exist an X such that $d(x)$ is positive for some $x \leqslant X$. It appeared later that this proof is a pure existence theorem and does not lead to any explicit numerical value of X ; such a numerical value, free of hypotheses, was found by Dr. Skewes in 1937 ; his work has not yet been published,† though it should be before very long. In the meantime I will report here on the matter, for there are unexpected features apart from the size of the final X.

If we denote by θ the upper bound of the real parts of the complex zeros of the Riemann ζ-function $\zeta(s)$, the famous 'Riemann hypothesis' (R.H. for short) is that $\theta = \frac{1}{2}$; if this is false, then $\frac{1}{2} < \theta \leqslant 1$. It has long been known that in the latter case $d(x) > x^{\theta - \epsilon}$ for arbitrarily small positive ϵ and some arbitrarily large x, so that an X certainly exists. This being so we may, for the purpose of a mere existence theorem, *assume* R.H., and my original proof did this. For a numerical X it is natural to begin by still assuming R.H. Doing this, Dr. Skewes found ‡ a new line of approach leading to

$$X = 10^{10^{10^{34}}}. \qquad (1)$$

* *See Math. Ann.* 109 (1934), 661–667; *Bull. Amer. Math. Soc.* 1928, 54–56; *Amer. Math. Monthly*, 43 (1936), 347–354.

† It is accessible in a Ph.D. thesis deposited in the Cambridge University Library.

‡ *See* J. L. M. S., 8 (1933), 277–283.

In this investigation it is possible to reduce the problem to a corresponding result about a function $\psi(x)$ associated with $\pi(x)$ [$\psi(x)$ is $\sum_{n \leqslant x} \Lambda(n)$, where $\Lambda(n)$ is $\log p$ if n is a prime p or a power of p, and otherwise 0]. The 'corresponding result' is

"$\delta(x) = \psi(x) - x - \frac{1}{2}x^{\frac{1}{2}} > 0$ for some $x \leqslant X$";

if $\delta(x) > 0$ for some x then (roughly—I simplify details) $d(x) > 0$ for the same x. Next we have (roughly) an "explicit formula" (R.H. or not)

$$\frac{\delta(x)}{x^{\frac{1}{2}}} = -\tfrac{1}{2} - \sum \frac{x^{\beta-\frac{1}{2}} \sin \gamma\eta}{\gamma}, \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2)$$

where $\beta + i\gamma$ is a typical complex zero of $\zeta(s)$ with positive γ, and we write $\eta = \log x$. Next, R.H. or not, a negligible error is committed by stopping the series in (2) at $\gamma \leqslant x^3$.

After these preliminaries we can consider the full problem of an X free of hypotheses, and the stages through which it went (this will involve a little repetition).

(i) Assume R.H. Then $\beta = \frac{1}{2}$ always, and it is a question of finding an $X = X_1$ such that

$$\sum_{\gamma \leqslant X^3} \frac{\sin \gamma\eta}{\gamma} > \tfrac{1}{2} \quad \text{for some } \eta \leqslant \log X.$$

The solution of this, which is a highly technical affair, is given in Dr. Skewes's J. L. M. S. paper. There is, as explained above, a final 'switch' from ψ to π (on well-established principles) and the X_0 arrived at is (1).

(ii) It is known that $\sum_{\gamma \leqslant T} 1/\gamma < A \log^2 T$ (it is actually of this order). If, instead of R.H., we assume slightly less, viz. that no zero with $\gamma < X_0^3$ has, say, $\beta \geqslant \frac{1}{2} + X_0^{-3} \log^{-3} X_0$, then

$\sum_{\gamma \leqslant X_0^3} x^{\beta-\frac{1}{2}} \frac{\sin \gamma\eta}{\gamma}$ differs trivially over the range $x \leqslant X_0$ from $\sum_{\gamma \leqslant X_0^3} \frac{\sin \gamma\eta}{\gamma}$,

and we still have (after trivial readjustments) the conclusion

"$\delta(x) > 0$ for some $x \leqslant X_0$".

(iii) It remains to prove the existence of a (new) X under the *negation* of the hypothesis in (ii). Now this negation is equivalent to the assertion of the existence of a $\beta_0 + i\gamma_0$ distant at least $b = X_0^{-3} \log^{-3} X_0$ to the right of $\Re s = \frac{1}{2}$, and with ordinate γ_0 not above X_0^3; *i.e.* we have a more or less *given* $\beta_0 + i\gamma_0$ with $\beta_0 - \frac{1}{2} \geqslant b$. Incidentally $\theta \geqslant \frac{1}{2} + b$ and there certainly *exists* an X. There is now a new surprise. With a $\beta_0 + i\gamma_0$ given as above we might reasonably expect the original θ-argument to provide an associated x with $\delta(x) > x^{\theta-\epsilon}$; alternatively, it is plausible that for *some* x the series $\sum_{\gamma \leqslant x^3} x^{\beta-\frac{1}{2}} \frac{\sin \gamma\eta}{\gamma}$ should exceed say 10^{-1} times the value of its individual term $x^{\beta_0-\frac{1}{2}} \frac{\sin \gamma_0\eta}{\gamma_0}$ (and with x making the sine positive). Dr. Skewes, however, convinced me that the argument does not do this: it does not deal in individual terms, and the difficulty is that any term we select may be interfered with by other terms of its own order or greater. The difficulty is not at all trivial, and some further idea is called for. In the end I was able, in general outline, to supply this.

(iv) But the problem is still not done for. Dr. Skewes convinced me (this time against resistance) that in the absence of R.H. it is no longer possible to

make the switch from ψ to π. This being so, it is necessary to carry through the work with the explicit formula for $\pi(x)$ instead of $\psi(x)$, with many attendant complications. This Dr. Skewes has done in his thesis, and his result stands at present as

$$X = N_4(1{\cdot}46).$$

§15. The problem just considered prompts the question: could there be a case in which, while pure existence could be proved, no numerical X could be given *because any possible value of X was too large to be mentioned.* The mathematician's answer is " no ", but we do thus return to the question, with which we began, of how large a number it is possible to mention. What we want is really a function $F(n)$ increasing as rapidly as possible ; what we finally substitute for n, whether 2, or u, or $N_u(u)$, makes no difference. (We must stop *somewhere* in constructing F, but one more step, say to $F(F(n))$, would overwhelm the difference in substitutions.)

We start with a strictly increasing positive $f_0(n)$. If we write $\psi^k(n)$ for the k-times iterated function $\psi(\psi(\ldots\psi(n))\,)$ we can define

$$f_1(n) = f_1(n, f_0) = f^{\cdot^{\cdot^{\cdot^{f(n)}}}}(n) \quad (\text{to } f_0(n) \text{ indices, say}),$$

where for clarity we have suppressed the zero suffixes in the right-hand terms.

This defines an increase of suffix from 0 to 1 ; we suppose f_2 derived similarly from f_1 (in symbols $f_2(n) = f_1(n, f_1)$), and so on. We now take a hint from the notation of transfinite ordinals, and form

$$f_{f_{\cdot^{\cdot^{\cdot^{f(n)}}}}(n)}(n)$$

(to $f_{f(n)}(n)$ suffixes, say). We can now say : scrap the existing definitions, as scaffolding, and define *this* to be $f_1(n)$, and carry on as before, We can scrap again, and so on : here I decide to stop. Once we stop we may take $f_0(n) = n^2$, or $n+1$ (*what* we take does not matter provided only $f_0(n) > n$).

The reader will agree that the numbers mentioned are large : it is not possible to say *how* large ; all that can be said about them is that they are defined as they *are* defined. If it were desired to compare terms in two rival systems a considerable technique would have to be developed. (It seems not quite trivial even to compare terms of

$$a^{a^{a^{\cdot^{\cdot^{\cdot}}}}} \quad \text{and} \quad b^{b^{b^{\cdot^{\cdot^{\cdot}}}}}.)$$

J. E. L.

1566. (806). Headline from *The Evening Standard*, July 25th, 1930 :

" Bradman, c. Duleepsinhji, b. Peebles, 14! "

If Bradman were to make 60 runs an hour, playing 6 hours a day, 6 days a week (allowing 52 such weeks to a year), he would complete his innings in 776,160 years. (Per Mr. James Buchanan.)

THE TRANSITION FROM SCHOOL TO UNIVERSITY MATHEMATICS.*

(A Schoolmaster's Point of View.)

By C. V. Durell.

A study of the examination papers for mathematical scholarships at Cambridge for the past forty years supplies probably the best guide to the changes which have occurred in the subject-matter and manner of presentation of VIth form mathematics because those in charge of school specialist courses, with few exceptions, are compelled or feel compelled to conform to the requirements of the universities as expressed either in their scholarship papers or in those for distinction in a higher certificate examination.

If you compare the Cambridge scholarship papers for, say, December 1916 with those for December 1946 you will notice some significant differences but also some equally significant resemblances.

More attention is now given to the fundamental principles of the calculus, and the range of work in analysis has altered in scope and has been extended, by stressing the concept of functionality. There is also an increased emphasis on the applications of the principles of mechanics to practical problems, and this forms a more severe test for the average pupil than was supplied by what may be called Loney-mechanics.

On the other hand, changes in the algebra of finite processes and in pure and analytical geometry are as yet scarcely noticeable, though it seems likely that these will become more evident in the near future and will in consequence influence VIth form work.

When I look back to my own school and university training at the beginning of this century, the feature which stands out conspicuously is the concentration of attack on special problems. It may be of interest to recall the textbooks in general use in schools at that time: Hall and Knight's *Higher Algebra*, Loney's *Trigonometry*, Smith's *Analytical Conic Sections*, Edwards' *Differential Calculus*, Williamson's *Integral Calculus*, Casey's *Sequel to Euclid*, Taylor's *Geometrical Conics*, Russell's *Pure Geometry*, Loney's *Statics and Elementary Dynamics*, Milne's *Problem Papers*.

The object was to secure technical facility in tackling a variety of types of problems, and these were all first-rate books for the purpose, with the possible exception of Casey's *Sequel* which was badly printed and more condensed than any other book I have come across, but with a wealth of material for those prepared to dig for it. In the last two terms at school after the scholarship examination, a start was made on such books as Smith's *Solid Geometry*, Forsyth's *Differential Equations*, Salmon's *Conic Sections*, Burnside's *Theory of Equations* and Routh's *Analytical Statics*.

This urge to tackle problems was maintained at Cambridge. There was no abrupt change in the character of the work; one merely continued with the same type of textbook as had been used at school. As an example of this drill in problem work, I will mention Herman's *Geometrical Optics*—a textbook written by a first-class teacher designed to prepare the reader for any sort of problem in geometrical optics which a Tripos paper might contain and a most efficient book for this purpose.

Success in the Math. Tripos, Part I, taken at the end of the 3rd year in the days of the Senior Wrangler and order of merit, depended on the ability to write out book-work rapidly and on a training which reduced to a minimum the time required to discover the best methods for solving problems. Most

* Presidential Address to the London Branch of the Mathematical Association, 1947.

lectures and the weekly problem-paper set by the College tutors or the private coach were directed solely to this end.

This was not a good training for the genuine mathematician : I think it was Professor Hardy who said on one occasion that he did not begin to learn mathematics till after he had taken his Tripos. It was not as dreary a course as it may sound, however open it may be to condemnation on educational grounds. There is a great deal of interest in problem-work and a solid sense of satisfaction in the discovery of a neat method of solution. But problem-work in mathematics bears much the same relation to mathematical thought as a chess problem does to the strategy and tactics of the game itself. Problem-work is indeed in the same class as solving cross-words and may be as seductive.

I will round off this account with what seems to me a perfect specimen of this problem era : it was devised by the late Professor Macdonald and included in the second problem paper of the Math. Tripos, Part I, 1902. Here it is :

"A thin spherical conducting shell from which any portions have been removed is freely electrified. Prove that the difference of the densities inside and outside at any point is constant."

Representative and perfect because (i) the statement is surprising, simple and contains the pseudo-practical flavour so common fifty years ago ; (ii) it is a hit or miss problem ; success or failure with no half-measures ; (iii) actually successes were very few, but those who solved it probably did so quickly.

It is of interest to compare this with a recent and, I think, perfect scholarship question taken from the Trinity group of papers for 1941, which illustrates a significant change of attitude :

"Five points in a plane are given, no three lying on a straight line. Prove that at least one of the quadrangles determined by a set of four out of the five points is convex."

I call this an ideal question because (i) it tests capacity rather than knowledge or special training ; (ii) it encourages the teacher to emphasise general mathematical principles rather than blind-alley conundrums ; (iii) a perfect answer might well by itself justify a scholarship award.

I have recalled this problem-solving routine because of its bearing on the transition from school to university work. In those days, the break came between Part I at the end of three years and the work for Part II by those who stayed up a fourth year.

The training for Part I was of little help for Part II ; the use of textbooks gave place to the study of papers in mathematical journals ; college lectures were replaced by university lectures which no longer ended with a series of problems on the subject-matter of the lecture, but with some general questions on the nature of the development and structure of the subject and references to current memoirs. But at the age of 21 or 22, a student was more mature and so better fitted to cope with a situation where the substance of a lecture could no longer be illuminated or supplemented by a formal textbook containing material in an easily digestible form.

The present-day Part I of the Math. Tripos taken in the first year is little more than an extension of VIth form work ; the difficulties of the transition press on those who take the Preliminary Examination instead of Part I and so jump straight into their professional mathematical training immediately they reach Cambridge. This is as it should be ; any university course should supply without delay the impetus for stimulating the mathematical imagination of the student by minimising the emphasis he may have been led to attach to special problems and directing his attention to broad principles. But it throws on the school-teacher a heavy responsibility. How can the concluding stages of the school work be arranged to smoothe the passage of the pupil when he reaches the university ?

Certainly not by incorporating into the school course any material part of the subjects of his first year's lectures; that would take the edge off the student's appetite and would be unwelcome for a variety of reasons to university teachers. It would confuse the distinct functions of school and university.

Broadly speaking, the function of the school is to illustrate the relation between mathematical processes and natural phenomena, and to provide such mathematical equipment as lies within the capacity of the pupil and is required for further progress.

Thus, main-school geometry is a branch of physics, not of pure mathematics; school mechanics is based mainly on appeals to experience, supplemented where practicable by quantitative experiments; the trigonometric functions are introduced as tools of the surveyor, not of the analyst; the first courses in algebra and calculus are founded on appeals to the eye.

Much of the time at school is spent in giving the pupil the tools he will need at a later stage; this involves some problem-work and the construction of a factual background. The value of this work will also depend on the extent to which the pupil is led to appreciate the inter-relation of the various fields of his activities. Our aim as teachers is to pursue mathematical education as a continuous growth, taking account of the increasing maturity of mind of the pupil.

How can this be done most effectively in the VIth form?

No answer is satisfactory which does not give adequate recognition to the central fact that for the *specialist* the transition from school to university is a transition from the viewpoint of physics to the concepts of pure mathematics. It is the task of the VIth form teacher to prepare the ground for the realisation of the meaning of pure mathematics and the recognition of the essential distinction between the elements of mathematics and the elements of the visible universe. The success of those who plan and conduct VIth form work must be judged by the extent to which their teaching, while avoiding any encroachment on the subject-matter of the university course, is impregnated with the spirit and ideas that characterise mathematical principles.

There can be no doubt that the book which has influenced most profoundly the specialist work at school on functionality is Hardy's *Pure Mathematics*, first published nearly forty years ago. This is much more than a textbook; it has been an inspiration both to teachers and pupils alike, and its pervasive influence is reflected in the transfer from emphasis on problem to emphasis on structure in the treatment of functionality in scholarship papers. It has contributed more than any other agency towards meeting the need for establishing continuity between school and university education in the particular, though wide, province with which the book is concerned. Yet perhaps more can still be done to guide the pupil along the track which Hardy's *Pure Mathematics* marks out. The pupil can be led by simple examples to appreciate the necessity for precise definitions of terms he has hitherto used in a loose or vague sense and to realise that mathematical propositions cannot be established by appeals to the eye. The theory of complex numbers is well within the capacity of the VIth form pupil and provides an ideal example of the necessity for new definitions of old symbols carried over into a new context; it also offers illustrations of another general mathematical method, the use of transformation. In this connection, I would urge with all the emphasis at my command that complex numbers should be excluded completely from *main-school* mathematics, where any attempt to treat them correctly is wellnigh impossible, with the result that anything which is likely to be said about them is injurious or even vicious; they do not belong either to elementary algebra or to elementary trigonometry, and their inclusion in these subjects is wholly pernicious.

Again, can we do more to meet the widespread complaint of university teachers that very few students who come to them have any idea of what is meant by convergence and talk a great deal of nonsense about it? Professor Hardy has emphasised that the ideas of convergence should be developed from first principles and that it is harmful to introduce any formal tests for convergence, still less the more refined ratio-tests, until the pupil has a firm grasp of their purpose. The danger is that a pupil tends to regard the determination of convergence of a series as an isolated exercise for which various mechanical tests are devised without appreciating the purpose of the enquiry. Instead of asking a pupil to prove that a series is convergent, we should ask him to consider what is the degree of accuracy in the approximation of the value of a function obtained from a given number of terms of a series. The meaning of convergence should in fact be explained by forcing the pupil to concentrate his attention on the remainder-function or on the discussion of upper and lower values between which the function is sandwiched. The premature use of ratio-tests is disastrous. It is indeed most unfortunate that the term, *infinite series*, is employed ; it is as misleading as the terms, real and imaginary.

For example, it encourages loose thinking by the student to talk about

$$1+x+x^2+\ldots$$

instead of talking about

$$s_n \equiv 1+x+x^2+\ldots+x^{n-1}. \quad \textit{Full stop.}$$

His attention should be concentrated on the behaviour of the function s_n when n increases. In this way, the pupil is led to associate the word convergence with the behaviour of the function of n, s_n, and not with the vague expression, infinite series. This is not a matter of playing with words ; it is vital in leading the pupil to appreciate the significance of the enquiry and is the only antidote for this talking-nonsense disease.

The purpose of these remarks is to show the extent of the debt which VIth form work owes to Hardy's book and to urge a still more extensive adoption in schools of its outlook on mathematics.

It is natural to go on to consider what would have been the effect on VIth form work if at the same time (1908) someone with Hardy's gift of stimulating presentation had written a companion volume on mathematical geometry, which would have shown this subject in broad outline and would have inoculated the school treatment with the ideas which permeate present-day methods of approach and attack.

The need for such a book is urgent. In the field of geometry there is a discontinuity of viewpoint between school and university which makes the word transition a misnomer. Professor Hardy showed in his book how the old-fashioned methods of the nineteenth century were making it necessary for students at the university to unlearn much of what they had been taught at school ; this is the present position in the field of geometry, and the pressing task of the schoolmaster is to re-create a course of geometry which will lead on smoothly to university methods by avoiding misunderstandings at present so often left to the university teacher to correct.

The fundamental distinction between school geometry and university geometry is, as I have said, that the former is a branch of physics and the latter a branch of mathematics.

Mathematical geometry is not a science of space ; it is not directly concerned with the facts of physical space nor does it formulate general laws to which spatial perception must conform. It does not and cannot prove anything about the physical world, whose structure can only be determined by observation. It is of course true that mathematical geometry may be able to

suggest a model which the structure of the universe resembles, but it rests with the physicist, not with the mathematician, to decide whether the suggestion has any value.

Confusion may also be caused by the fact that mathematicians and physicists use the same words in fundamentally distinct senses. For example, in common cartesian geometry, the definition of the length of the line AB joining (x_1, y_1) to (x_2, y_2) is $\sqrt{\{(x_1 - x_2)^2 + (y_1 - y_2)^2\}}$, and this definition is precise ; it is algebraic, not spatial. But a physicist's statement of a length is inevitably only an approximation, not only on account of experimental errors but also from inability to determine precisely what is being measured and what his measurement represents.

At school, we associate metrical geometry, which deals with such magnitudes as length, size of angle, area, etc., with properties that are unchanged by the motion of a so-called rigid body. We associate the geometry of parallel projection, translation and rotation with properties that are unchanged by homogeneous deformations of an elastic body. We associate projective geometry with the principles of perspective in pictorial representation. We associate topological geometry with properties that are unchanged if an elastic surface is stretched and twisted in any manner provided only that no two distinct points are brought into actual coincidence and that either no cut or tear is made or, if made, the edges of the cut are finally united in exactly the same way as at first. In each of these modes of study, we are dealing with spatial concepts and material bodies ; these are not mathematical geometries.

In fact, school geometry is a mongrel breed and cannot be otherwise as long as it is a branch of physics. This is not a ground for condemnation ; probably no school teacher would wish it to be otherwise ; but the position must be recognised and it should be part of the VIth form course to lead the pupil to realise it and so far as is practicable to appreciate the various meanings of the word geometry in mathematics.

Consider for example Desargues' theorem : " If the joins of corresponding vertices of two triangles are concurrent, then the meets of corresponding sides are collinear."

This theorem forms part of the VIth form course and the pupil will study various proofs and profit by doing so. But it is far more instructive, though more difficult, for the pupil to consider what result is in fact established by any particular proof and to examine the various assumptions on which the proof is based.

It may be a theorem in pure projective geometry. In the geometry of three dimensions, the proof is simple if the triangles are not coplanar, and not unduly difficult if the triangles are coplanar. But in the geometry of two dimensions, the statement, or some equivalent, is incapable of proof and becomes an axiom. The first reaction of the pupil is to say that anyhow there *are* three dimensions, so why all the fuss? That may be so in the universe, but not necessarily in a mathematical geometry, which has just as many dimensions as we care to postulate. Again, it may be a theorem in analytical projective geometry. In this case the proof is comparatively simple.

In this way the pupil is led to realise that a statement in geometrical language may have wholly different and unconnected meanings, and may be true in some geometries and incapable of proof in others. In fact, the statement in an examination syllabus that methods either of pure geometry or of analytical geometry may be employed for proving a proposition really means that the candidate has the choice either of proving a certain theorem in some system of pure geometry or another theorem in some system of analytical geometry, the only connection between the theorems being that they are expressed by the same words, which however carry different meanings.

F. W. KELLAWAY

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There is no reason why the scholarship candidate should not realise that there are different mathematical geometries and anyhow he should be able to appreciate the fundamental distinction between an analytical geometry and a pure geometry.

An appreciation of the essence of an analytical geometry is well within the capacity of the VIth form pupil ; it is simply a branch of analysis concerned with the properties of sets of numbers. It consists solely of definitions and theorems.

For example, in analytical projective plane geometry, the ordered triad (x, y, z) where x, y, z are not all zero is called a point and the numbers x, y, z are called its coordinates.

We say that $$(x_1, y_1, z_1) = (x_2, y_2, z_2)$$

if and only if a number k (not zero) exists such that

$$x_1 = kx_2, \quad y_1 = ky_2, \quad z_1 = kz_2,$$

and we say that the triads are the same point. The ordered triad $[X, Y, Z]$ is called a straight line and the numbers X, Y, Z are called its coordinates.

We say that $$[X_1, Y_1, Z_1] = [X_2, Y_2, Z_2],$$

if and only if a number k (not zero) exists such that

$$X_1 = kX_2, \quad Y_1 = kY_2, \quad Z_1 = kZ_2,$$

and we say that the triads are the same line. We say that (x, y, z) is *on* $[X, Y, Z]$ and that $[X, Y, Z]$ is *on* (x, y, z), if and only if

$$Xx + Yy + Zz = 0.$$

If for short we write P_r in place of (x_r, y_r, z_r) we can then *prove* the theorems that

(i) there is one and only one line on two distinct points P_1, P_2 and we call it the join of P_1 and P_2, and

(ii) If P_3, P_4 are distinct points on the join of the distinct points P_1, P_2, then P_1, P_2 are distinct points on the join of P_3, P_4.

We see also that the principle of duality is a direct consequence of our definitions.

It would be outside the scope of this address to pursue this work in detail, nor is it necessary for my purpose to do so. The point I wish to make is that these are not spatial theorems and their proof cannot depend on reference to or the appearance of a diagram.

There are no axioms in analytical geometry ; the sole purpose of an axiom is to limit the subject-matter, and for an analytical geometry the subject-matter is a precise branch of analysis.

An appreciation of the essence of a pure geometry makes greater demands on the pupil because we start off with entities to which we give names—any names will do—but which are never defined ; say a system of entities we call points from which we select partial aggregates we call lines and other aggregates we call planes, and so on ; and then define the meaning of such phrases as " lie on ", " point and line figures ", etc.

The particular geometry we proceed to study is then limited by the set of axioms we select. Any set of axioms which are consistent and independent is permissible. The theorems of this geometry are the sets of logical relations between the undefined entities imposed by the selected axioms.

It is the province of the university to examine and compare various sets of axioms, but the VIth form pupil should appreciate the meaning and function of an axiom, and it may be both stimulating and instructive to examine the

content of a pure geometry based on some very simple set of axioms. He can and should realise, for example, that pure projective geometry does not start where main-school elementary geometry leaves off, that is to say, it does not make use of any results of elementary physical geometry. It starts from scratch and studies the logical relations set up by the chosen axioms. Consequently a procedure in which we use properties of the circle to deduce projective properties of the conic is out of step with the development of the subject. The circle is studied as a special form of the conic ; the conic is not regarded as a generalised form of the circle. Thus we may consider a projective theorem of the conic and then examine what is the corresponding property of the circle, *not* vice versa.

For example, consider the theorem : " If P, P' are corresponding points of two homographic ranges on a conic, the envelope of PP' is a conic having double contact with the given conic."

In analytical projective geometry, the proof is very short and simple.

In pure projective geometry, the proof is neither short nor easy, but the corresponding property of the circle, as a theorem in metrical geometry, is merely an illustration of symmetry.

There are an unlimited number of geometries and these are as distinct from one another as the rooms in a house, though any particular room may be subdivided into two rooms by the erection of another wall, that is by the addition of another axiom ; or two rooms may be thrown into one by the removal of a wall, that is by the removal of an axiom.

It is also well within the range of VIth form work for the pupil to realise that although a theorem in an analytical geometry may be expressed by precisely the same words as a theorem in a pure geometry, these are distinct theorems, and the fact that a statement has been proved to be true in one geometry does not imply that it is also a true theorem in another geometry : it may be true or false or meaningless or even one of the selected axioms.

Progress along the lines I have indicated makes large demands on the skill and wisdom of the teacher, but no greater than those which have already been made and satisfied in the years since the Mathematical Association was founded. It will render less abrupt the sharp change in mental outlook between school and university and will remove the present reproach that it is often necessary for a student to start his university course by curing himself of habits of thought acquired by his school training. C. V. D.

1567. (9). RHAPSODISTS. (Vieta) states that he gets his denominations from certain *Raphsodi* (as he calls them ; it is not often that mathematical tabulators are called rhapsodists) whom he does not name.—De Morgan, *Tables*, P.C., Supp. 29, p. 597.

1568. (824). And if the Greek Muses wer a graceful company
yet hav we two, that in maturity transcend
the promise of their baby-prattle in Time's cradle,
Musick and Mathematick : coud their wet-nurses
but see these foster-children upgrown in full stature
Pythagoras would marvel and Athena rejoice.

—Robert Bridges, *The Testament of Beauty*, 1.737. [Per Miss M. O. Stephens.]

NEWTON AND THE ATTRACTION OF A SPHERE.

By J. E. Littlewood.

1. In Keynes's contribution to the 'Newton Tercentenary Celebrations' there is the following passage:

"Again, there is some evidence that Newton in preparing the *Principia* was held up almost to the last moment by lack of proof that you could treat a solid sphere as though all its mass were concentrated at the centre, and only hit on the proof a year before publication. But this was a truth which he had known for certain and had always assumed for many years.

"Certainly there can be no doubt that the peculiar geometrical form in which the exposition of the *Principia* is dressed up bears no resemblance at all to the mental processes by which Newton actually arrived at his conclusions.

"His experiments were always, I suspect, a means, not of discovery, but always of verifying what he knew already."

To know things by intuition happens to humbler people, and it happened, of course, to Newton in supreme degree; but I should be inclined to doubt this particular example, even in the absence of evidence. Many things are not accessible to intuition at all, the value of

$$\sum_{r=0}^{n} ({}_nC_r)^3,*$$

for instance. The 'central' attraction of a sphere is, of course, more arguable, but in point of fact Newton says in his letter to Halley of 20th June, 1686, that until 1685 (probably early spring) he suspected it to be false. (For the letter, see Rouse Ball, *An Essay on Newton's Principia*, pp. 156–159; the critical passage is from l. 7 on, p. 157. See also p. 61.) There is, I think, a sufficient and fairly plausible explanation for the proof being held up, and an analysis of the mathematical setting of the problem is not without interest.

2. I take it as established that Newton did not believe in the central attraction before 1685. This being so he may well have thought the determination of the actual attraction a detail to be considered later. (It is true that in the 1666 comparison of the attraction at the moon's distance with that at the surface of the earth, the departure from centrality in the latter case, being at its worst, would be a serious matter. This, however, merely adds further mystery to a subject already sufficiently obscure. It is odd, incidentally, that all the accounts of the matter that I have come across before 1947 ignore this particular point.) He did, however, attack the problem in the end. Now, *with a knowledge of the answer*, the problem reduces at once to the attraction of a spherical *shell*, which in straightforward integration in cartesians happens to reduce to integration of the function $(ax+b)/(cx+d)^{\frac{3}{2}}$, child's play to Newton. *Without* this knowledge it is natural to attack the solid sphere; this is more formidable, and may have baffled him until he had fully developed his calculus methods. To the Newton of 1685 the problem was bound to yield in reasonable time: it is possible (though this is quite conjectural) that he tried the approach *via* a shell of radius r, to be followed by an integration with respect to r; this would of course instantly succeed. Anyhow he found a proof. But this was by no means the end of the matter. What he had to find was, as we all know, a proof, no doubt a calculus one in the first instance, which would 'translate' into geometrical language. Let the reader try.

I think we can infer with some plausibility what the calculus proof was, and I give it in modern dress. It operates, of course, on the *shell*.

* For which there is a formula!

3. The figure is Newton's except that I have added the dotted line SH and marked three angles.

Let $SH = a$, $SP = r$. The variable of integration is taken to be ϕ.* Consider the zone generated by rotation of HI round SP, and its (resolved) contribution $\delta F = \delta F_P$ to the total attraction F_P on P. This contribution satisfies

$$r^2 \frac{\delta F}{\delta \phi} = \left(\frac{PS}{PI}\right)^2 \cos\psi \,.\, 2\pi a \mid \delta\theta \mid IQ \,.\, \frac{1}{\delta\phi},$$

$$\frac{r^2}{a^2}\frac{dF}{d\phi} = \left(\frac{\cos\phi}{\sin\theta}\right)^2 \sin(\theta+\phi)\sin\theta \left|\frac{d\theta}{d\phi}\right| \quad \ldots\ldots\ldots\ldots\ldots\ldots (1)$$

From triangle PHS we have

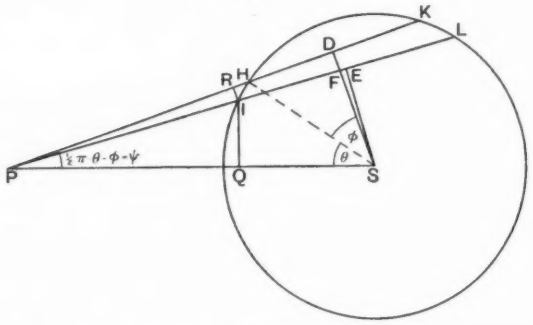
$$\frac{\cos(\theta+\phi)}{\cos\phi} = \frac{a}{r},$$

whence

$$-\frac{d\theta}{d\phi} = 1 - \tan\phi\cot(\theta+\phi) = -\frac{\sin\theta}{\sin(\theta+\phi)\cos\phi} \quad \ldots\ldots\ldots\ldots\ldots\ldots (2)$$

From (1) and (2)

$$\frac{r^2}{2\pi a^2}\frac{dF}{d\phi} = \cos\phi. \quad \ldots\ldots\ldots\ldots\ldots\ldots (3)$$



The range R of ϕ is $-\frac{1}{2}\pi$ to $\frac{1}{2}\pi$, and we should arrive at the appropriate result by integration. Newton's argument, however, is in effect that R is independent of P, so that for two positions P, p, we have $F_P/F_p = Sp^2/SP^2$.†

4. Now for the geometrical proof (which must have left its readers in helpless wonder). There is a counterpart figure in small letters (the two spheres being equal). The use of ϕ-integration translates into the 'peculiar' idea "let phk cut off an arc hk equal to HK, and similarly for pil". The details that follow are in essentials elegant and very carefully arranged, but the archaic language makes for heavy going, and I modernise. The *difficulties* arise from (2), naturally troublesome to translate.

Since $hk = HK$ and $il = IL$, we have

$$se = SE, \quad df(= sd - se = SD - SE) = DF. \quad \ldots\ldots\ldots\ldots (4)$$

The contributions $\delta F_{P,\,p}$ of the two incremental zones of HI, hi satisfy

$$\frac{\delta F_p}{\delta F_P} = \frac{PI^2}{pi^2} \cdot \left(\frac{pf}{ps}\Big/\frac{PF}{PS}\right) \cdot \frac{hi \,.\, iq}{HI \,.\, IQ} \quad \ldots\ldots\ldots\ldots (5)$$

* The order of lettering significantly corresponds to a *positive* increment $\delta\phi$.

† This is the statement of the crucial 'Prop. 71'. The constant is actually never determined.

Also
$$\frac{pf \,.\, PI}{pi \,.\, PF} = \frac{pf}{pi} \Big/ \frac{PF}{PI} = \frac{df}{ri} \Big/ \frac{DF}{RI} = \frac{RI}{ri}$$
$$= \frac{IH}{ih}, \quad \text{.....(6)}$$

since $\angle RHI = \angle rhi$. From $PI/PS = IQ/SE$, its counterpart, and $SE = se$, we have
$$\frac{PI}{PS} \Big/ \frac{pi}{ps} = \frac{IQ}{iq}. \quad \text{.....(7)}$$

Multiplying (6) and (7) we have
$$\frac{PI^2 \,.\, pf \,.\, ps}{pi^2 \,.\, PF \,.\, PS} = \frac{IH \,.\, IQ}{ih \,.\, iq},$$

which combines with (5) to give
$$\frac{\delta F_p}{\delta F_P} = \frac{PS^2}{ps^2}.$$

5. There is a proof satisfying Newton's canons, which one feels he might have arrived at if he had found no other way. It arises easily enough out of the modern approach and is as follows : it operates on the solid sphere. (The original notation P, S is being continued.)

Consider Σ, the concentric sphere through P, the component attraction $N_Q(P)$ at P normal to Σ of a unit particle at Q, and the average $\overline{N}_Q$ of $N_Q(P)$ over all P of Σ. The contribution to the total $4\pi a^2 \overline{N}_Q$, *i.e.* average multiplied by area, of an element of area $\delta\Sigma$ is, by easy geometry, the solid angle subtended by $\delta\Sigma$ at Q. Hence $4\pi a^2 \overline{N}_Q = 4\pi$, and $\overline{N}_Q$ is independent of Q and so equal to $\overline{N}_S$. Now $\Sigma \overline{N}_Q \delta V_Q$, taken over elements of volume of the solid sphere, is the average normal attraction of the solid sphere taken over points of Σ, and its equivalent $\overline{N}_S \Sigma \delta V_Q$ is the corresponding thing for the mass concentrated at S. In each case the thing averaged is constant and equal to the *total* force, and the equivalence gives what we want.

6. Return to the question of ' intuition '. The obvious argument ' against ' is : " What has the inverse square got that the inverse cube hasn't ? " There is an answer to this : the inverse square is the ' natural ' law of diminution with distance, *e.g.* of light or sound, and others than Newton thought of it in connection with the planetary system.* Alternatively, adopt a corpuscular theory of light, in which corpuscles from different origins do not collide. If they are, say, inelastic, the corpuscles from a point create a *repulsion* according to the inverse square, and *the total pressure on a sphere with the point as centre is independent of the radius*. This independence, combined with the symmetry, might produce the feeling that the situation in the original attraction problem is best compatible with ' centrality ' : an intuition. But by this time we are within striking distance of the proof given above. The total outward (*i.e.* normal) pressure on a Σ due to an origin of corpuscles at Q is a not unnatural idea, and the key is to prove that it is independent of the position of Q.

J. E. L.

* It may be observed that with a law r^{-0} the attraction at the surface of the solid sphere is less than the central value, while with a law r^{-4} it is greater. These facts are obvious, in the second case because the attraction concerned is infinite.

THE BASIC LEMMA IN MULTIPLICATIVE DIOPHANTINE ANALYSIS.

By E. T. Bell.

1. Multiplicative diophantine analysis is concerned with the complete integer solution of systems of sets of equations, each set of the type

$$x_1^{a_1} \dots x_r^{a_r} = y_1^{b_1} \dots y_s^{b_s} = \dots = z_1^{c_1} \dots z_t^{c_t},$$

where the $a, b, \dots, c$ are positive integers and the $x, y, \dots, z$ are the unknowns. The solution is found by repeated application of the basic lemma that the complete integer solution of the equation (1·1) is (1·2), in which a, b, f, k are integer parameters ranging independently over the ring C of rational integers:

$$xy = zw. \qquad (1\cdot1)$$

$$x = af,\ y = bk,\ z = ak,\ w = bf\ ;\ f, k \text{ may be taken relatively prime.} \qquad (1\cdot2)$$

It can then be shown that (1·2) is the complete solution of (1·1) in any Euclidean ring. Hence in such rings the solution of multiplicative systems is formally the same as in C. We shall prove the like for any integral domain R (commutative ring *with a unity element* e and no nilfactors) in which all ideals are principal, by showing that (1·2) is the complete integer solution of (1·1) in R as a, b, f, k range independently over R. For C, the two units $1\,(=e)$ and -1 are implicit in the parameters in (1·2), and need not be explicitly noted. It is to be proved * that the like is permissible in R, where there may be an infinity of units.

Decomposition into primes in R is unique up to permutations *and unit factors*, and any two elements in R have a G.C.D. which is unique *up to unit factors*. Thus the introduction of ideals into R is superfluous and, with some care, ideals can be avoided in solving (1·1) in R. However, it is as short to use as to avoid them, and their use makes it quite clear that there is no possible fallacy of the kind noted by Landau,* where the source of error in claiming completeness for partial solutions of certain diophantine equations in C is imprecision respecting the units in unique factorisation domains. The domain D defined by $r^2 - 2 = 0$, for which prime decomposition and G.C.D.'s are unique up to unit factors, furnishes an example. The equation $x^2 - 2 = y^3$ in C is multiplicative in D; the error originates in confusing element-coprimality with ideal-coprimality in D, either notationally or otherwise. On account of the numerous applications of the lemma it is desirable that some proof or other of it be recorded for reference.

The following algebraic details will be required. Greek letters denote units in R; $\epsilon\epsilon^{-1} = e$. The principal ideal generated by the element u of R is written (u); the unit ideal is (e); $(\epsilon) = (e)$; $(uv) = (u)\,(v)$. The G.C.D. of (h) of (u), (v) is the uniquely determined ideal written $((u), (v))$. If $((u), (v)) = (e)$, the ideals (u), (v) are said to be coprime. If the G.C.D. of the elements u, v is an arbitrary unit, u, v are said to be element-coprime. If u, v are any elements of R, u/v (u divides v) if and only if $(u)/(v)$. If $(u) = (v)$, u, v are associates $u = \epsilon v$.

2. For (1·1) in R, $(xy) = (zw)$,

$$(x)\,(y) = (z)\,(w). \qquad (2\cdot1)$$

* For the relevant algebra, see A. A. Albert, *Modern Higher Algebra* (1937), Chapter II; B. L. van der Waerden, *Moderne Algebra*, 2nd edition (1937), vol. I, §§17–19. For the connection of (1·1), (1·2) with the fundamental theorem of arithmetic in C, see L. Kalmár, "Über den Fundamentalsatz der Zahlentheorie", *Math. phys. lapok*, vol. 43 (1936), pp. 27–45. For the necessary caution regarding neglect of units, see E. Landau, *L'Intermédiare des Math.*, vol. 8 (1901), pp. 145–7, and L. E. Dickson, *History of the Theory of Numbers*, vol. 2 (1920), pp. 534, 536. For multiplicative equations, see Th. Skolem, *Diophantische Gleichungen* (1938), Kap. 4.

Let $((y), (w)) = (g)$. Then

$$(y) = (g)(y_1), \quad (w) = (g)(w_1), \quad ((y_1), (w_1)) = (e)\ ; \quad \ldots\ldots\ldots\ldots (2{\cdot}2)$$

$$(x)(y_1) = (z)(w_1) \ldots\ldots\ldots\ldots (2{\cdot}3)$$

There are only the possibilities

$$(y_1) = (e), \quad (w_1) = (e)\ ; \ldots\ldots\ldots\ldots (2{\cdot}41)$$

$$(y_1) = (e), \quad (w_1) \neq (e)\ ; \ldots\ldots\ldots\ldots (2{\cdot}42)$$

$$(y_1) \neq (e), \quad (w_1) = (e)\ ; \ldots\ldots\ldots\ldots (2{\cdot}43)$$

$$(y_1) \neq (e), \quad (w_1) \neq (e). \ldots\ldots\ldots\ldots (2{\cdot}44)$$

If (2·41), then from (2·3), $x = \epsilon_1 z$, and from (2·2), $y = \epsilon_2 g$, $w = \epsilon_3 g$. If these are to satisfy (1·1) in R, $\epsilon_3 = \epsilon_1 \epsilon_2$. The solution may be written in the form (1·2) with

$$a = z, \quad b = \epsilon_2 g, \quad f = \epsilon_1, \quad k = e. \ldots\ldots\ldots\ldots (2{\cdot}5)$$

If (2·42), then from (2·3), $x = \epsilon_1 z w_1$. From (2·2), $y = \epsilon_2 g$, $w = \epsilon_3 g w_1$. If these are to satisfy (1·1) in R, $\epsilon_3 = \epsilon_1 \epsilon_2$. The solution may be written in the form (1·2) with

$$a = \epsilon_1 z, \quad b = \epsilon_1 \epsilon_2 g, \quad f = w_1, \quad k = \epsilon_1^{-1}. \ldots\ldots\ldots\ldots (2{\cdot}6)$$

By symmetry, (2·42), (2·43) are essentially the same. Either thus or independently, the solution when (2·43) holds is of the form (1·2) with

$$a = \epsilon_1 x, \quad b = \epsilon_1 \epsilon_2 g, \quad f = \epsilon_1^{-1}, \quad k = y_1. \ldots\ldots\ldots\ldots (2{\cdot}7)$$

If (2·44), then from the last of (2·2), no divisor $\neq (e)$ of (y_1) divides (w_1). Hence by (2·3), $(y_1)/(z)$. Similarly, $(w_1)/(x)$. Thus

$$(z) = (y_1)(y_2), \quad (x) = (w_1)(w_2).$$

It follows from (2·3) that $(w_2) = (y_2)$. Hence, and from (2·2)

$$(x) = (y_2 w_1), \quad (y) = (g y_1), \quad (z) = (y_2 y_1), \quad (w) = (g w_1)\ ;$$

$$x = \epsilon_1 y_2 w_1, \quad y = \epsilon_2 g y_1, \quad z = \epsilon_3 y_2 y_1, \quad w = \epsilon_4 g w_1.$$

If these are to satisfy (1·1) in R, $\epsilon_1 \epsilon_2 = \epsilon_3 \epsilon_4$; $\epsilon_4 = \epsilon_1 \epsilon_2 \epsilon_3^{-1}$. The solution may be written in the form (1·2) with

$$a = \epsilon_1 \epsilon_3 y_2, \quad b = \epsilon_1 \epsilon_2 g, \quad f = \epsilon_3^{-1} w_1, \quad k = \epsilon_1^{-1} y_1. \ldots\ldots\ldots\ldots (2{\cdot}8)$$

By (2·5)–(2·8), all solutions of (1·1) in R are given by (1·2) as a, b, f, k range over R. It is readily seen that (2·5)–(2·7) are included in (2·8). For example, if in (2·8)

$$y_2 = \epsilon_1^{-1} \epsilon_3^{-1} \epsilon_1' z', \quad g = \epsilon_1^{-1} \epsilon_2^{-1} \epsilon_1' \epsilon_2' g', \quad w_1 = \epsilon_3 w', \quad y_1 = \epsilon_1 \epsilon_1'^{-1},$$

the result is (2·6) with $\epsilon_1, \epsilon_2, g, w_1, z$ replaced by $\epsilon_1', \epsilon_2', g', w', z'$. E. T. B.

1569. (249). I should like to hear about Grassmann—whom I am not likely to read. Between ourselves, I am disappointed with Germans—almost always. I have a new theory—take it. German intellect is an excellent thing, but when a German product is presented it must be analysed. Most probably it is a combination of intellect (I) and tobacco smoke (T). Certainly $I_3 T_1$ and $I_1 T_1$ occur ; but $I_1 T_3$ is more common, and $I_2 T_{15}$ and $I_1 T_{20}$ occur. In many cases Metaphysics occurs (M) ; and I hold that $I_a T_b M_c$ never occurs without $b + c > 2a$.—A. De Morgan to W. R. Hamilton, Graves' *Life of Hamilton*, iii, p. 446. [Per Professor R. W. Genese.]

THE TRICIRCULAR GENERATION OF A FOCUS-SHARING TRIAD OF CONICS.

By E. H. Neville.

"Didst thou never study the mathematics?"
"What's that, sir?"
"Why, to know the trick how to make a many lines meet in one centre."
Duchess of Malfi.

Let α, β, γ be three conics, one with foci B, C, one with foci C, A, and one with foci A, B. The specification of a single conic as the locus of the centre of a directed circle which touches two fixed directed circles is not unique but involves both a parameter and a choice of sign. Can we adjust the arbitrary elements in specifying α, β, γ to obtain a simple specification of the focus-sharing set as a whole? Explicitly, if α is derivable from circles η', ζ' round B, C, β from circles ζ'', ξ'' round C, A, and γ from circles ξ''', η''' round A, B, what conditions can be usefully imposed on the concentric pairs ξ'', ξ'''; η''', η'; ζ', ζ''?

It is evident in advance that although the parameters are involved linearly, we can not impose coincidence on the directed circles, for if α, β, γ could be derived from three circles ξ, η, ζ, the centre of a circle touching these three circles would be a point common to the three conics. Analytically, any three directed circles have two contact-centres, but if two of the conics are given, the third conic may be any conic with the assigned foci, and there is no reason whatever why it should pass through even one point of intersection of the first two: the specification of a general set can not have quite the simplicity which would lead inevitably to concurrence.

Let us examine the specification algebraically. Without loss we may suppose α to be given in the first place by a point circle at B and a circle (C, h), β by a point circle at C and a circle (A, f), and γ by a point circle at A and a circle (B, g). For equivalent determinations involving parameters, η', ζ' have radii x, $x+h$; ζ'', ξ'' have radii y, $y+f$; and ξ''', η''' have radii z, $z+g$. The equations

$$y+f=z, \qquad z+g=x, \qquad x+h=y$$

are not independent, and are usually inconsistent, as we have foreseen. But if we reverse the signs on the right, we have equations

$$y+f=-z, \qquad z+g=-x, \qquad x+h=-y,$$

which are always consistent, and from these we can derive a geometrical theory in which fundamental concentric circles, though different, differ only in direction.

Writing $f+g+h=2k$, we have the solution

$$x_0=-k+f, \qquad y_0=-k+g, \qquad z_0=-k+h,$$

and we denote the circles round A, B, C with radii x_0, y_0, z_0 by ξ_0, η_0, ζ_0, and the reversed concentric circles, whose radii are $-x_0$, $-y_0$, $-z_0$, by $-\xi_0$, $-\eta_0$, $-\zeta_0$. Then α is the locus of the centre of a directed circle which touches $-\eta_0$ and ζ_0; β is the locus of the centre of a directed circle which touches $-\zeta_0$ and ξ_0; and γ is the locus of the centre of a directed circle which touches $-\xi_0$ and η_0. This is the tricircular generation of the focus-sharing set α, β, γ from ξ_0, η_0, ζ_0.

Given f, g, h, the tricircular basis is unique, but for given conics α, β, γ the signs of f, g, h are independently arbitrary. To change the three signs simultaneously is only to reverse the directions of all the circles involved, but

to change one sign and not the others is to change the circles completely. Hence there are four essentially distinct modes of tricircular generation of a given triad ; each of these can be modified trivially by a reversal of cyclic direction throughout, but there are no other possibilities. We denote the complete scheme of radii as follows :

$$\begin{aligned} x_0,\ y_0,\ z_0 &= -k+f,\ -k+g,\ -k+h,\\ x_1,\ y_1,\ z_1 &= \ -k,\ \ k-h,\ \ k-g,\\ x_2,\ y_2,\ z_2 &= \ k-h,\ \ -k,\ \ k-f,\\ x_3,\ y_3,\ z_3 &= \ k-g,\ \ k-f,\ \ -k, \end{aligned}$$

and we extend the notation for the directed circles themselves in the same way.

Since β is derivable from ζ_0 and $-\xi_0$, and γ from $-\xi_0$ and η_0, a point P which is the centre of a circle ϖ touching the three circles $-\xi_0, \eta_0, \zeta_0$ is a point of intersection of β and γ. If R_0 is the radical centre of the three undirected circles $|\xi_0|, |\eta_0|, |\zeta_0|$, and Π_0 is the power of R_0 for each of these circles, the reverse of the inverse of ϖ for centre R_0 and constant Π_0 is a second circle touching $-\xi_0, \eta_0, \zeta_0$, and the centre of this circle is a second intersection Q of β and γ. The two centres P, Q are in line with the centre of inversion R_0.

If the circle round P with radius r touches the circles round A, B, C with radii $-x_0, y_0, z_0$, the circle round P with radius $r+f$ touches the circles round A, B, C with radii $-x_0+f, y_0+f, z_0+f$, that is, with radii $-x_1, y_1, z_1$. Thus the contact circles of the set $-\xi_1, \eta_1, \zeta_1$ determine the same points of intersection of β and γ as the contact circles of the set $-\xi_0, \eta_0, \zeta_0$, and the line joining these points of intersection passes through the radical centre R_1 as well as through the radical centre R_0. That is to say, the line R_0R_1 is a chord of intersection of the conics β, γ and it passes through real common points if the sets of circles with which we have associated it have real contact circles.

With an appropriate direction of measurement parallel to AB, the distance from the radical axis of $-\xi_0$ and η_0 to the directrix of γ which corresponds to A as focus is * $x_0(x_0+y_0)/AB$. Hence the ratio of the distances of the radical centres R_0, R_1 from this directrix is $x_0(x_0+y_0) : x_1(x_1+y_1)$, and similarly the ratio of the distances of these centres from the directrix of β which corresponds to A as focus is $x_0(x_0+z_0) : x_1(x_1+z_1)$. But $x_0+y_0 = -h = x_1+y_1$ and $x_0+z_0 = -g = x_1+z_1$. Hence the two ratios are equal, and the line R_0R_1 passes through the intersection of the two directrices.

The substitution of $-g$ for g replaces x_0, y_0, z_0 by x_3, y_3, z_3 and x_1, y_1, z_1 by $-x_3, -y_3, -z_3$. It follows that the line R_2R_3 also passes through the intersection of the same directrices, and that this line also is a chord of intersection of β and γ. But the line is distinct from R_0R_1, as we can see by examining conditions of contact. Let (P, r) touch $-\xi_2, \eta_2, \zeta_2$. Then, because (P, r) touches $(A, -x_2)$ and (B, y_2), $(P, r+x_2-y_2)$ touches $(A, -y_2)$ and (B, x_2), that is, touches $(A, -x_1)$ and (B, y_1). Also, because (P, r) touches $(A, -x_2)$ and (C, z_2), $(P, r+x_2-z_2)$ touches $(A, -z_2)$ and (C, x_2), that is, touches (A, x_0) and $(C, -z_0)$, and therefore $(P, -r-x_2+z_2+f)$ touches $(A, -x_0+f)$ and (C, z_0+f), that is, touches $(A, -x_1)$ and (C, z_1). But the two circles $(P, r+x_2-y_2)$, $(P, -r-x_2+z_2+f)$ are not identical ; since the sum of their radii is $2k$, that is, $-2x_1$, they are the two distinct circles round P which touch $-\xi_1$, and what we have shown is that one of them touches η_1 and the other touches ζ_1. Thus P is recognisable from the set $-\xi_1, \eta_1, \zeta_1$ as a point lying on both β and γ, but P is not a contact-centre of this set and is therefore not one of the points on the chord R_0R_1.

* Note 1309, *Gazette*, XXII, 378.

To sum up :

A focus-sharing triad of conics can be generated tricircularly in four distinct ways. Each pair of sides of the quadrangle formed by the radical centres of the four bases is a pair of common chords of two of the conics, and the corresponding diagonal point is the intersection of the two directrices associated with the common focus of these two conics. Three sides of the quadrangle which have one of the radical centres in common are three concurrent common chords of the set of conics.

The conclusion as to concurrence has become trivial, and it will not escape notice that since the radical centres are real points, this theorem determines the relevant common chords of the conics as real lines, whether or not there are real points of intersection on them. E. H. N.

CORRESPONDENCE.

EXPERIMENT AND THE TEACHING OF MECHANICS.

To the Editor of the *Mathematical Gazette*.

SIR,—In his very interesting address of April, 1947, the retiring President has the passage : " It seems incredible that I finished a three years' course at the university in Mathematics without having entered a laboratory either at school or university, and yet we were taught subjects such as Hydrostatics, Optics, Electricity, Mechanics, and the like."

I deny that there is absurdity so far as Mechanics and Hydrostatics are concerned. The rightful prestige of the *thing* 'experiment' is no reason for being awestruck or browbeaten by the *word*, and I think some august persons and examining bodies have been. There are two points. One is that merely by living the ordinary man has built up a system of intuitions on which basic ideas of Mechanics can be legitimately founded. The other is that there are such things as *mental* experiments; Hydrostatics for example is full of them.

I do not of course assert that experiment in the teaching of Mechanics is necessarily valueless. It may make the relevant idea more vivid to a boy whose mental grasp is poor, and it may set up a useful association to make an idea stick in the early stages (so, indeed, might an apposite—or an inapposite—Stock Exchange story). And so on. But if a clever boy does find watching experiments a bore and a waste of time, he is entirely within his rights. (I believe the idea was tried in Cambridge, with the natural result.)

I may add that I expressed these views recently in a company of mathematicians and scientists, and not only was there no dissent, but one scientist volunteered the tentative opinion that some elementary science was in the same case. Yours, etc., J. E. LITTLEWOOD.

1570. (70). THE EDUCATIONAL VALUE OF EXPERIMENT. It may be said that the fact makes a stronger impression on the boy through the medium of his sight, that he believes it the more confidently. I say that this ought not to be the case. If he does not believe the statements of his tutor, probably a clergyman of mature knowledge, recognised ability, and blameless character —his suspicion is irrational, and manifests a want of the power of appreciating evidence, a want fatal to his success in that branch of science which he is supposed to be cultivating.—Todhunter, *The Conflict of Studies*, 1873, p. 17.

THE ELEMENTARY MATHEMATICS OF THE ROCKET.

By R. C. Knight.

A very great amount of scientific effort has been expended in the last few years on Rocket research and development. This work has been carried out in many countries besides Great Britain, notably the U.S.A., Germany and Russia. In the main it has been done by government scientists or by firms and universities under government contract. This is understandable since the chief use of rockets so far has been a military one. There is a very long history of the use of rockets in warfare (they were used at Waterloo), culminating in the production and firing of the German V2. Most of the major rocket weapons produced in the last war have been described in general terms in the Press, but there is, at present, remarkably little mathematical or scientific literature on the subject. Various technical journals, *e.g. Flight*, have published articles on the various aspects of the rocket problem, and there are one or two books. This lack is due to the fact that most of the papers written during the war years are classified as Secret or Confidential and are not yet available for open publication. Two recent papers are worthy of mention : one by Perring (1) on German Long Range Rocket Development, which gives a considerable amount of data on the V2 and one by Malina and Summerfield (2) on Rocket Escape from the Earth which discusses the possibilities of very large rockets. These may be regarded as the forerunners of very many papers which should eventually find their way into scientific literature.

In this note I propose to indicate, in the most elementary manner, some of the basic principles which are used in the mathematical theory of rockets. No claim is made of originality, except in presentation, since the ideas expressed are all either very elementary or have been in use for many years. It is, however, felt that a paper of this character may be of interest to teachers and students alike, as an example of the application of relatively elementary mathematics and mechanics to a real and practical problem.

I acknowledge with gratitude the permission of the Chief Scientist of the Ministry of Supply to communicate this paper.

1. *The Rocket.*

It is inappropriate here to enter into design details of rockets. but it may be as well to specify the most general characteristics from which we formulate the basic equations. The rocket is a self-contained projectile which relies for its propulsive power on a fuel and oxidant carried with it. In this respect it differs from a shell, where the charge is entirely external to the projectile or from any device which requires an external oxidant, *e.g.* a V1. The propellant may be solid, *e.g.* gunpowder or cordite ; or it may be liquid, *e.g.* liquid oxygen and petrol. The propellant is ignited by a suitable device and burns in a combustion chamber ; the resulting gases escape through a nozzle. It is the momentum of these gases, expelled backwards at very high speed, which produce in the remainder of the rocket the increased momentum which propels it forwards. The rocket can therefore operate in a vacuum or region of low air density. We may thus consider, in the first instance, a body of varying mass, a portion of which is thrown backwards at a known speed independent of external conditions. A more detailed consideration will show that the pressure of the atmosphere into which the gases are expanded by the nozzle has some effect, but this is small.

2. *The Point Motion of the Rocket.*

We first consider the motion of the rocket as a point in space acted upon

by its own thrust, gravity and aerodynamic forces. To take the simplest case, we ignore gravity and the air reactions, which are small forces compared with the thrust in many rockets, and consider the behaviour during burning.

Let $M(t)$ be the mass of the rocket at time t,
$V(t)$ its velocity at that time,
$\dot{m} = dm/dt$, the mass rate of expulsion of the gases,
ω the gas expulsion (or efflux) velocity relative to the rocket.

At time t the momentum is MV, and since $dM = -dm$, the momentum at time $t+dt$ of the rocket is $(M-dm)(V+dV)$. The gases of mass dm ejected backwards will have momentum $dm(V-\omega)$, and hence

$$(M-dm)(V+dV) - MV - dm(\omega - V) = 0,$$

which reduces to

$$dV = \omega \dot{m}\, dt/M. \quad \ldots\ldots(1)$$

If we assume that the rocket starts from rest and M_0 is its initial mass, m being the mass of the propellant consumed, and t_0 the time of burning velocity at "all burnt" V_b, *i.e.* the maximum velocity, can be found by integrating equation (1), provided ω is constant. We have

$$V_{\max} = V_b = \int_0^{t_0} \frac{\omega \dot{m}}{M}\, dt = -\int_{M_0}^{M_0 - m} \frac{\omega}{M}\, dM$$

$$= \omega \log_e \frac{M_0}{M_0 - m}. \quad \ldots\ldots(2)$$

It is usual to write α as the ratio of the fuel weight to the total weight, so that

$$V_{\max} = -\omega \log_e (1-\alpha). \quad \ldots\ldots(3)$$

Returning to equation (1), we define the instantaneous thrust T as

$$T = M\frac{dV}{dt} = \omega \dot{m},$$

and note that

$$\frac{d}{dt}(MV) = \dot{m}(\omega - V). \quad \ldots\ldots(4)$$

A standard parameter in rocket work is the specific impulse S, defined as the thrust in lb.wt. per lb. per sec. of the propellant, and hence

$$S = T/\dot{m}g = \omega/g. \quad \ldots\ldots(5)$$

We may therefore write (3) in the alternative forms :

$$V_{\max} = -(T/\dot{m}) \log (1-\alpha) = -gS \log (1-\alpha). \quad \ldots\ldots(6)$$

We next widen the problem by introducing gravity and air resistance but, with the assumption of no yaw, ignore aerodynamic side forces. If we resolve the forces acting on the rocket, tangentially and normally, we obtain :

$$M\frac{dV}{dt} = \dot{m}\omega - R - Mg \sin\theta, \quad \ldots\ldots(7)$$

$$MV\frac{d\theta}{dt} = -Mg\cos\theta, \quad \ldots\ldots(8)$$

in which θ is the angle between the tangent to the trajectory and the horizontal and R the air resistance in the form

$$R - \tfrac{1}{2}\rho V^2 s\, C_D. \quad \ldots\ldots(9)$$

Here ρ is the air density, s the projected area of the rocket, and C_D the " drag coefficient ".

The above equations (7) and (8) are used to set up a numerical iteration process of integration to obtain the trajectory. It is not proposed to discuss the details of the various processes, which are laborious and require a detailed knowledge of the particular rocket to obtain the various quantities, *e.g.* C_D, necessary to the computation. We may remark that for preliminary calculations R may be neglected compared with T for rockets having a high acceleration, *e.g.* the A.A. rocket. Also, if the rocket is very large, R may still be neglected even if accelerations are small, *i.e.* comparable with g without appreciable error in the result. The case of vertical flight is of interest and gives an example of the variation in final velocity with the rate of burning of the propellant.

Let us take equation (7), neglect R, and write

$$M\frac{dV}{dt}=\dot{m}\omega - Mg.$$

Integrating this from zero conditions we obtain as in (2):

$$V_{\max}=-\omega\log(1-\alpha)-gt_b, \quad \text{.....(10)}$$

where t_b is the time of burning. If ng is the maximum nett acceleration upwards attained by the rocket, then for a rocket with constant thrust,

$$\dot{m}t_b=M_0\alpha\,;$$

while

$$M_0\frac{(1+n)}{(1-\alpha)}g=T=\dot{m}\omega,$$

which gives gt_b in terms of n, α, ω, and equation (10) becomes

$$V_{\max}=-\omega\{\log(1-\alpha)+\alpha/(1-\alpha)(n+1)\}. \quad \text{.....(11)}$$

We may alternatively postulate a rocket in which $\dot{m}$ is controlled to produce a thrust at all times proportional to the mass. *i.e.* a constant acceleration rocket. It is left to the reader to establish that in this case, for vertical flight,

$$V_{\max}=-\frac{\omega n}{1+n}\log(1-\alpha). \quad \text{.....(12)}$$

It is, of course, possible to postulate a number of idealised problems neglecting air forces—most of these will readily occur to the reader. For example, for long-range rockets, where the distance travelled during burning is small, we may assume $V_{\max}$ as the initial velocity for an " in vacuo " parabolic trajectory, and so determine the qualitative behaviour of range with variations in the basic parameters ω and α.

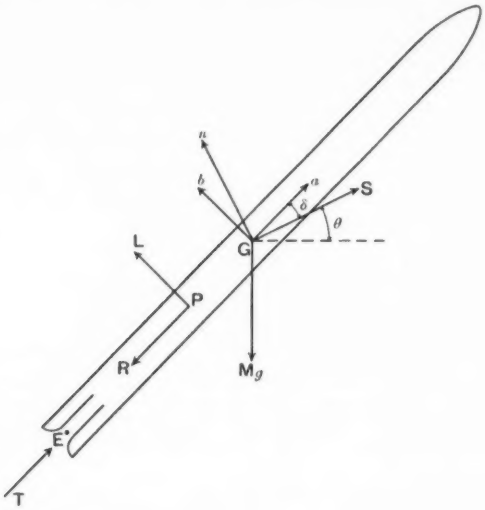
3. *The Rigid-body Motion of the Rocket.*

The three-dimensional problem of rocket motion in its most general form is one of some complexity. It has received considerable attention in recent years. In this paper we shall not attempt even to formulate the most general equations, but shall content ourselves with discussing, in a simplified form, the two-dimensional motion.

We assume the rocket to be a body of the character indicated in Fig. 1, stabilised by fins and not spinning about its longitudinal axis.

The points G, P, E denote the c. of g., c. of p., and centre of exit (of the gases). Let $l=GE$, $d=GP$. The unit vectors $(\vec{a}, \vec{b})$, $(\vec{s}, \vec{n})$ specify the directions along and perpendicular to the rocket axis and trajectory respec-

tively. Let δ = yaw of the rocket, θ the trajectory inclination and $\phi = \delta + \theta$ respectively.



The forces acting are :

(i) The rocket thrust, acting at E, due to $\dot{m}$ expelled at velocity $-\vec{\omega a}$ relative to E, while E has velocity $\vec{Vs} - l\dot{\phi}\vec{b}$. This force (see equation (4)) is thus

$$\vec{T} = \dot{m}\{\omega\vec{a} - \vec{Vs} + l\dot{\phi}\vec{b}\}.$$

(ii) External force of gravity Mg vertically downwards.

(iii) The aerodynamic forces of lift (L) and resistance (R) in directions $\vec{n}$ and $-\vec{s}$ respectively. R is usually small compared with the thrust and will later be neglected. L has the convenient form $L = KV^2\psi$, where K is a constant and ψ is the effective angle of incidence. It will be seen that $\psi = \delta + \delta'$ where δ' is the angle between the velocities of G and P, and is approximately $d\dot{\phi}/V$ since $l\dot{\phi}$ is small compared with V. Hence

$$L = KV^2(\delta + d\dot{\phi}/V) \text{ approximately.}$$

The moments of these forces about an axis through G are found to be $-\dot{m}l^2\dot{\phi}$ and $-dL$.

If $I(t)$ is the appropriate moment of inertia, we may write the equations of motion as

$$\frac{d}{dt}(M\vec{Vs}) = \dot{m}\{\omega\vec{a} - \vec{Vs} + l\dot{\phi}\vec{b}\} + L\vec{n} - R\vec{s} + M\vec{g}, \quad \ldots\ldots\ldots\ldots(14)$$

and

$$\frac{d}{dt}(I\dot{\phi}) = -\dot{m}l^2\dot{\phi} - dL. \quad \ldots\ldots\ldots\ldots(15)$$

Taking resolutes in directions s and n, putting $\sin\delta = \delta$ and $\cos\delta = 1$ and remembering that $\dot{M} = -\dot{m}$, we may deduce from (14) the following two equations :

$$M\dot{V} = \dot{m}\omega - \dot{m}l\dot{\phi}\delta - R - Mg\sin\theta, \quad \ldots\ldots(16)$$

$$MV\dot{\theta} = \dot{m}\omega\delta + \dot{m}l\dot{\phi} + KV^2(\delta + d\dot{\phi}/V) - Mg\cos\theta, \quad \ldots\ldots(17)$$

while (15) becomes

$$I\ddot{\phi} + \dot{I}\dot{\phi} = -\dot{m}l^2\dot{\phi} - KdV^2(\delta + d\dot{\phi}/V). \quad \ldots\ldots(18)$$

These equations do not admit a simple solution. Even after burning has ceased and the equations reduce to those of a shell (without spin), the solution cannot be obtained without resource to ballistic tables on account of the varying resistance function. It is, however, possible to approximate for a given rocket when physical quantities are known and thereby deduce a number of results of qualitative value in rocket research. Any approximations we make are, in general terms, applicable to a rocket similar to the A.A. rocket, *i.e.* one with high thrust and high initial acceleration.

To a first approximation, neglecting all forces except the thrust, we have from (16),

$$\dot{V} = \dot{m}\omega/M \; ; \quad \ldots\ldots(19)$$

which enables us to rewrite (17) as

$$(MV - \dot{m}l - KVd)\dot{\phi} = MV\dot{\delta} + (M\dot{V} + KV^2)\delta - Mg\cos\theta,$$

or since $\dot{m}l$ and KVd are small compared with MV,

$$V\dot{\phi} = V\dot{\delta} + (\dot{V} + KV^2/M)\delta - g\cos\theta. \quad \ldots\ldots(20)$$

To simplify the notation, write $n^2 = Kd/l$ and

$$\lambda = (\dot{I} + \dot{m}l^2 + Kd^2\dot{V})/I,$$

so that (18) may be written

$$\ddot{\phi} + \lambda\dot{\phi} + n^2V^2\delta = 0. \quad \ldots\ldots(21)$$

It is convenient to use s, the intrinsic trajectory coordinate, in place of t and to use a new variable defined as

$$\eta = V\delta.$$

Making these substitutions and eliminating ϕ between equations (20) and (21), we may obtain

$$\frac{d^2\eta}{ds^2} + \left(\frac{\lambda}{V} + \frac{K}{M}\right)\frac{d\eta}{ds} + \left\{n^2 + \frac{\lambda K}{MV} + \frac{d}{ds}\left(\frac{K}{M}\right)\right\}\eta = g\left\{\frac{\lambda\cos\theta}{V^2} + \frac{d}{ds}\left(\frac{\cos\theta}{V}\right)\right\}. \quad \ldots(22)$$

If we write $\chi = \eta e^{\frac{1}{2}\int_{s_0}^{s}\left(\frac{\lambda}{V} + \frac{K}{M}\right)ds} = \eta e^{A(s)}$, (22) is transformed to

$$\frac{d^2\chi}{ds^2} + n^2\chi = +eA\,g\left\{\frac{\lambda\cos\theta}{V^2} + \frac{d}{ds}\left(\frac{\cos\theta}{V}\right)\right\}. \quad \ldots\ldots(23)$$

$$= +eA\,Bg.$$

Here terms additive to n^2 multiplying χ have been neglected, since they may be shown to be small compared with n^2 provided the rocket is stable. Returning to (20), we deduce

$$\frac{d\theta}{ds} = \frac{\eta}{V^3}\left(\dot{V} + \frac{KV^2}{M}\right) - g\frac{\cos\theta}{V^2}. \quad \ldots\ldots(24)$$

Finally the deviation of the rocket from its original line of projection, the quantity of most interest, is given by

$$D=\int_{s_0}^{s}\theta\,ds. \qquad\qquad (25)$$

This completes the formal solution, it being possible to express θ and D in terms of integrals involving the functions χ, A and B. It is not proposed to go further with the general solution here ; in any case a further discussion of the behaviour of I with time and a knowledge of the values of the various quantities, *e.g.* n, $\dot{m}$ would be necessary.

Numerical methods have to be employed to solve many of the problems of interest, but it is possible to reduce the labour by suitable approximations in certain cases. Of these the effect of wind on a burning rocket is one of importance, and an indication of the method is given as an example. Other problems that can be dealt with include the effect of (*a*) initial conditions, *e.g.* yaw, angular velocity, travel along a projector ; (*b*) a malaligned thrust. Problems of type (*a*) can be solved directly from the equations given above with suitable approximations. That of (*b*) requires $\omega\vec{a}$ replaced by $\omega\vec{a}'$ at a small angle to $\vec{a}$ and the later equations modified accordingly.

5. *Example. The Effect of Wind on a Rocket.*

For this example we neglect gravity and consider the deviation from the motion in the vertical plane, due to a constant cross wind of velocity w, normal to the line of fire. The deviation is known to be small, so that we may take w as normal to the trajectory at all times ; *i.e.* $w\vec{n}$. This velocity is assumed always small compared with V after projection, so that the air flow velocity is taken as V, but the effective angle of incidence of the rocket is now $\psi+w/V$. This affects only the terms in KV^2, in which δ is replaced by $\delta+w/V$.

Equation (22) is therefore replaced by :

$$\frac{d^2\eta}{ds}+2\frac{dA(s)}{ds}\frac{d\eta}{ds}+\left(n^2+\frac{\lambda K}{MV}+\frac{d(K/M)}{ds}\right)(\eta+w)=0. \qquad\qquad (26)$$

Now let $\chi_w=(\eta+w)e^{A(s)}$ and, as before, obtain

$$\frac{d^2\chi_w}{ds^2}+n^2\chi_w=0. \qquad\qquad (27)$$

The initial conditions are $\delta_0=\dot{\phi}_0=s_0=0$, from which may be deduced :

$$A(0)=0, \quad \eta_0=0\ ; \quad (\chi_w)_0=w, \quad (d\chi_w/ds)_0=wA_1(0)$$

(the last by using equation (20)) in which $A_1=\frac{1}{2}(\lambda/V-K/M)$.

The required solution to satisfy these conditions is

$$\chi_w=(V\delta+w)e^{A(s)}=w(\cos ns+(A_1/n)\sin ns),$$

from which we deduce the yaw,

$$\delta=-\frac{w}{V}\left(1-e^{-A(s)}\left(\cos ns+\frac{A_1}{n}\sin ns\right)\right). \qquad\qquad (28)$$

In this example we have

$$\frac{d\theta}{ds}=\frac{\delta\dot{V}}{V^2}+\frac{K}{M}\left(\delta+\frac{w}{V}\right)=\frac{\delta\dot{V}}{V^2}+\frac{K}{MV}\chi_w e^{-A(s)}\ ;$$

Photo: Dalby

T. A. A. BROADBENT

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and hence, since $\theta_0 = \delta_0 = 0$,

$$\theta = -\int_0^s \eta\, d(1/V) + \int_0^s \frac{K\chi_w}{MV} e^{-A(s)}\, ds$$

$$= \delta + \int_0^s \frac{d\eta}{ds}\frac{1}{V}\, ds + \int_0^s \frac{K\chi_w}{MV} e^{-A(s)}\, ds$$

$$= \delta + \int_0^s \frac{d\chi_w}{ds} e^{-A(s)} \frac{ds}{V} - \int_0^s A_1(s)\chi_w e^{-A(s)} \frac{ds}{V}. \quad \ldots\ldots\ldots\ldots\ldots(29)$$

The deviation D is given by

$$D = \int_0^s \theta\, ds,$$

$$= \int_0^s \delta\, ds + \int_0^s \int_0^s \frac{d\chi_w}{du} e^{-A(u)} \frac{du}{V}\, ds + \int_0^s \int_0^s A_1(u)\chi_w e^{-A(u)} \frac{du}{V}\, ds$$

$$= \int_0^s \delta\, ds + \int_0^s \frac{d\chi_w}{du} e^{-A(u)}(s-u)\frac{du}{V} - \int_0^s \chi_w(1 + (s-u)A_1(u)) e^{-A(u)} \frac{du}{V}. \ldots(30)$$

A useful qualitative result in a much simpler form can be obtained by further, rather ruthless, approximations, which are, however, of the same order as those used above. We assume a constant acceleration f, perfect damping, *i.e.* neglect terms in dA/du and also the terms in A_1 compared with adjacent terms. For ease of computation we normally use the dimensionless parameter v defined by $v = \sqrt{\frac{nV}{\pi f}}$ with $s = \pi v^2/2n$, etc. Hence the yaw is simply

$$\delta = -\frac{w}{V}(1 - \cos ns)$$

$$= -\frac{w}{V}\sqrt{\frac{n}{\pi f}}\left\{1 - \cos\frac{\pi}{2}v^2\right\}.$$

From (29),

$$\theta = -\frac{w}{V}\{1 - \cos ns\} - \int_0^s \frac{wn}{V} \sin ns\, ds,$$

$$= -w\sqrt{\frac{n\pi}{f}}\left\{\frac{1}{\pi v}\left(1 - \cos\frac{\pi v^2}{2}\right) + S(v)\right\}.$$

Which from (30),

$$D = s\theta + \int_0^s \frac{nw}{V} s \sin ns\, ds - \int_0^s \frac{w}{V} \cos ns\, ds$$

$$= s\theta + \frac{w}{2}\sqrt{\frac{\pi}{nf}}\{v - C(v)\}.$$

In the above, $C(v)$ and $S(v)$ are the Fresnel Integrals (3):

$$C(v) = \int_0^v \cos\frac{\pi v^2}{2}\, dv, \qquad S(v) = \int_0^v \sin\frac{\pi v^2}{2}\, dv.$$

It is noteworthy that the deviation is in the direction opposite to w, *i.e.* the rocket, when under the jet reaction, deviates into the wind.

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1. " Critical Review of German Long Range Rocket Development," W. G. A. Perring, *Jour. R. Ae. Soc.*, 1946.
2. " Problem of Escape from the Earth by Rocket," F. J. Malina and M. Summerfield, *California Institute of Technology Report* (presented at the VIth Cong. App. Mechs., Paris, 1946).
3. Fresnel Integrals : see E. Jahnke and F. Emde tables.

(Since the above paper was written a new book on this subject has appeared: *The Mathematical Theory of Rocket Flight*, by J. B. Rosser, R. R. Newton, and G. L. Gross, and published by the McGraw-Hill Book Company of New York. A review of this book appears on pp. 222-3 in this *Gazette*.) R. C. K.

1571. (**195**). Besant's *Notes on Roulettes and Glissettes*, 1870, probably the first or only mathematical book ever noticed in *Punch*. Of this Besant was justly proud, for the work was his own, even to the word " Glissette ", which he invented, and he had the cutting from *Punch* pasted into his own copy :

WHAT CAN THEY BE? *Punch*, 2nd April, 1870.

Notes on Roulettes and Glissettes.—This sounds rather frisky for the title of a book by a " Lecturer and Fellow of St. John's College, Cambridge ", and we should feel easier as to the future of St. John's College, if we could receive an assurance from the College Authorities that they have examined the work in question, and can vouch for its being of the highest respectability.

(A manuscript note by W. J. G. asserts that this was sent to *Punch* by the Reverend E. W. Bowling, Fellow of St. John's.)

1572. (**37**). MARY SOMERVILLE. At the age of 92 she writes : At this time I had the pleasure of a visit from Mr. Pierce, Professor of Mathematics and Astronomy, in the Harvard University, U.S. . . . He kindly sent me a beautiful lithographed copy of a very profound memoir in linear and associative algebra. Although in writing my popular books I had somewhat neglected the higher algebra, I have read a great part of the work ; but as I met with some difficulties I wrote to Mr. Spottiswoode, asking his advice as to the books that would be of use, and he sent me Serret's *Cours d'Algèbre Supérieure*, Salmon's *Higher Algebra*, and Tait on *Quaternions* ; so now I got exactly what I wanted, and I am very busy for a few hours every morning ; delighted to have an occupation so entirely to my mind.—*Personal Recollections of Mary Somerville*, edited by her daughter, p. 356.

1573. (**46**). Das Leben der Götter ist Mathematik. Alle göttlichen Gesandten müssen Mathematiker sein. Reine Mathematik ist Religion. Die Mathematiker sind die einzig Glücklichen. Der echte Mathematiker ist Enthusiast *per se*. Ohne Enthusiasmus, keine Mathematik. Echte Mathematik ist das eigentliche Elemente des Magiers. In der Musik erscheint sie förmlich als Offenbarung, als schaffender Idealismus. Hier legitimiert sie sich als himmlische Gesandte, *kat'anthropon*. Das höchste Leben ist Mathematik. Zur Mathematik gelangt man nur durch eine Theophanie.

Wer ein mathematisches Buch nicht mit Andacht ergreift und es wie Gottes Wort liest, der versteht es nicht.—Novalis, *Schriften*, p. 223. [Per Prof. H. G. Forder.]

CONICS WHICH TOUCH FIVE GIVEN CONICS.

By A. Robson.

In these days of examinations and syllabuses it is perhaps not superfluous to remind teachers of the maxim that they ought to teach more than their pupils need to learn. To be an expert on circles it is necessary to study other conics, and to appreciate homographies it is as well to know something about other correspondences.

The ideas of correspondence and characteristics of systems of conics are due to the French mathematician Chasles (*c.* 1850) and a large collection of illustrations of these subjects may be found in T. Lemoyne's *Les Lieux Géométriques* (Vuibert, 1923). These illustrations include, though in a summary form, the subject of the present article. Reference may also be made to the last example in C. Taylor's *Ancient and Modern Geometry of Conics*, and to a note in Salmon's *Conic Sections*.

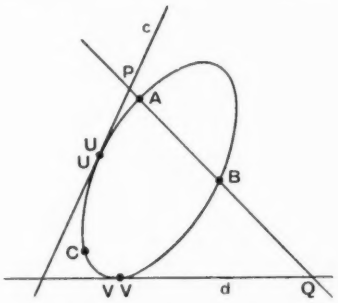
The conics which pass through p given points and touch q given lines and r conics where $p+q+r=4$ form an ∞^1 system Σ which may be denoted by $[pqr]$.

$[pqr]$ has three characteristics : ξ, or $\xi[pqr]$, is the number of conics of the system which pass through a given point ; η, or $\eta[pqr]$, is the number which touch a given line ; and ζ, or $\zeta[pqr]$, is the number which touch a given conic.

The conditions for the conic $ax^2+by^2+cz^2+2fyz+2gzx+2hxy=0$ to pass through 5 given points are 5 linear equations for the 5 independent ratios of a, b, c, f, g, h, and so, in general, just one conic passes through 5 given points. Hence $\xi[400]=1$.

Conics through 4 given points meet a given line in pairs of points in involution ; and so just two of them touch the line at the double points of the involution. Hence $\xi[310]=\eta[400]=2$.

A slightly more elaborate application of the involution theorem gives the values of $\xi[220]$ and $\eta[310]$. For consider the system of conics through two fixed points A, B touching two fixed lines c, d ; let U, V be the contacts of one



of the conics with c, d, and let P, Q be the points in which AB meets these lines. Then the involution theorem applied to conics through U, U, V, V shows that UV passes through one of two fixed points on AB, namely the double points of the involution determined by the pairs A, B and P, Q. If the conic also passes through a third fixed point C, it can be shown in the same way that UV passes through one of two fixed points on AC, and so UV has four possible positions. Conversely, it can be shown that for each position

there is a conic through A, B, C touching c, d. Hence $\xi[220]=\eta[310]=4$. By the principle of duality, the remaining values of ξ and η for the systems in which $r=0$ can be deduced. They are shown in the table below, and a formula for ζ in terms of ξ, η will be found by means of some preliminary lemmas.

	ξ	η	ζ
[400]	1	2	6
[310]	2	4	12
[220]	4	4	16
[130]	4	2	12
[040]	2	1	6

(i) The locus of the pole of a given line wo conics of a system Σ is a curve of order η.

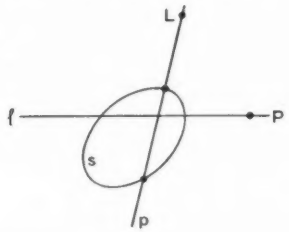
For the given line touches η of the conics, and the pole of a line lies on the line if and only if the line touches the conic. Hence η points of a given line belong to the locus.

(ii) Dually, the envelope of the polar of a given point wo conics of a system Σ is a curve of class ξ.

(iii) The locus of a point P whose polar wo a given conic coincides with its polar wo some conic of a given system Σ is a curve of order $\xi+\eta$.

This is proved by means of a correspondence that is not one-to-one.

Let s be the given conic, l a fixed line, L the pole of l wo s; and let P be a point on l, and p its polar wo s. Thus p passes through L.



First suppose that P is fixed. By (i) the poles of p wo conics of the system Σ lie on a curve of order η. This curve meets l in η points Q. Each point Q has the property that its polar (p) wo some conic of Σ is also the polar of P wo s. For the fixed position of P on l, there are η points Q on l with this property.

Now suppose that Q is a fixed point on l. By (ii) the polars of Q wo conics of Σ envelop a curve of class ξ. There are ξ of these polars through L, and each of them is also the polar wo s of a point P which lies on l. For the fixed position of Q there are ξ positions of P.

Hence there is a (ξ, η) correspondence between the points P and Q on l which have the property that the polar of P wo s coincides with the polar of Q wo some conic of Σ. The general equation of a (ξ, η) correspondence shows that there are $\xi+\eta$ points of l in which P coincides with Q. These are the points which have the same polar wo s and wo some conic of Σ. This proves (iii).

The value of ζ can now be found. For let s be a given conic, A a point on it, and t the tangent to s at A. If and only if A has the same polar (t) wo s and wo a conic of a system Σ, this conic will also touch t and therefore s at A. By (iii) the locus of such points is of order $\xi + \eta$, and so there are $2(\xi + \eta)$ of the points on s. Hence $\zeta = 2(\xi + \eta)$.

It now follows that for the systems [301], [211], [121], [031], the values of ξ are 6, 12, 16, 12; and the values of η are 12, 16, 12, 6; and hence the values of ζ are 36, 56, 56, 36.

In the same way for [202], [112], [022], the values of ξ are 36, 56, 56; the values of η are 56, 56, 36; and the values of ζ are 184, 224, 184. Again, for [103], [013], the values of ξ are 184, 224; of η, 224, 184; and of ζ, 816, 816.

Lastly, for [004], $\xi = \eta = 816$, and $\zeta = 3264$. Thus there are in general 3264 conics which touch five given conics, and a table like that given above can be constructed for the remaining systems [*pqr*]. A. R.

1574. (67). More specially, I appoint that five of the "John Welsh Bursaries" shall be given for best proficiency in Mathematics (I would rather say "in Mathesis", if that were a thing to be judged of from competition), but practically, above all, in pure geometry, such being perenially the symptom, not only of steady application, but of a clear methodic intellect, and offering, in all epochs, good promise for all manner of arts and pursuits.—Carlyle's Bequest of Craigenputtoch to the University of Edinburgh.

1575. (124). A yerd she hadde . . .
In which she hadde a cok, hight Chauntecleer,
In al the land of crowing nas his peer . . .
Wel sikerer was his crowing in his logge,
Than is a clokke, or an abbey orlogge.
By nature knew he ech ascencioun
Of equinoxial in thilke toun;
For whan degrees fiftene were ascended,
Thanne crew he, that it mighte nat ben amended.

—Chaucer, *The Nonne Prestes Tale*, 14853. [Per Sir G. Greenhill.]

1576. (270). Even more severe was the sentence passed (by Valentinian and Valens, A.D. 365?, *Cod. Theod.* xvi, 5.3) against the hapless mathematicians. In words which would now carry terror through the pleasant places by the Cam, the Imperial brothers decreed: "Let the discourse of the Mathematicians cease. For if in public or in private, by night or by day, anyone shall be caught instructing another in this forbidden error, both the teacher and taught shall be sentenced to capital punishment. For it is no less a crime to teach than to learn forbidden arts." By Mathematicians were doubtless here meant Astrologers.—Hodgkin, *Italy and her Invaders*, i, pp. 204–5.

1577. (414). Small need then to say as a wind up that arithmetic and algebra in their wonted setting forth cannot but be educationally bad and mischievous scientifically misleading bewildering unhelpful balking stunning deadening and killing and philosophically worthless.—Preface to *Pelicotetics*, A. Sandeman, 1867. [Per Professor E. H. Neville.]

1578. (630). The lithe and sinewy Wilson—smiling—once a Senior Wrangler, says someone—who conjures for us with sine and secant and plays all sorts of pretty tricks with angles.—C. Mosely, *Cornhill*, Feb. 1915. (On Canon J. M. Wilson, one of the founders of our Association.)

PROOF BY REDUCTIO AD ABSURDUM.*

By R. L. Goodstein.

If, like David Copperfield, you are on the road to Dover, for the first time and with no one to guide you, and you come to a point where the road forks, then of all your mathematical knowledge, only the method of *reductio ad absurdum* can help you. For if you choose the wrong turning, and find yourself in Folkestone, you may be quite sure you ought to have taken the other road.

Of course you may well consider that it would have been far better to have chosen the right road at the outset, and that is exactly how I feel about a *reductio ad absurdum* proof.

I am going to follow the usual practice in mathematics of calling any indirect proof, a proof by *reductio ad absurdum*, though there are indirect proofs, quite commonly met with in mathematics, which are not *reductions to a contradiction* ; these are based on the formula

$$(\textit{not-}q \text{ implies } \textit{not-}p) \text{ implies } (p \text{ implies } q), \qquad \text{(F)},$$

that is, q follows from p if you deny p by denying q.

For instance, to prove

$$\text{if } \tfrac{1}{2}(1+(-1)^n) \text{ is odd then } n \text{ is even}$$

it suffices, by formula (F), to observe that

$$\text{if } n \text{ is odd then } \tfrac{1}{2}(1+(-1)^n)=0, \text{ which is even ;}$$

this is an indirect proof, but there is no apparent reduction to a contradiction.

The genuine *reductio ad absurdum* is a way of setting out an indirect proof which we have inherited from Euclid. In the last example, for instance, following Euclid, we should make two hypotheses :

(i)	$\tfrac{1}{2}(1+(-1)^n)$ is odd,
(ii)	n is odd,
and from (ii) deduce	$\tfrac{1}{2}(1+(-1)^n)$ is even,

which contradicts the first hypothesis. The conclusion is that the hypotheses cannot both be true, *i.e.* the first hypothesis implies that the second is false. In modern logic we should call the hypotheses *axioms* and would say that a formal system postulating both the axioms (i) and (ii) contains a contradiction, and conclude that the axioms are incompatible.

Though different in form these two methods of proof, the first by means of formula (F), and the other by incompatible axioms, are logically equivalent, and I shall take the latter as the standard form.

Consider the following general scheme of a *reductio ad absurdum* proof :

axiom or proved proposition,	A
hypothesis,	*not-p*
theorem,	*not-p* implies *not-A*
conclusion,	p.

Evidently, the scheme is based on two presuppositions. In the first place we are assuming that A cannot be both true and false, for unless we reject the *contradiction* A and *not-A*, we have not made a case against *not-p*, and

* Lecture delivered to the General Meeting of the Mathematical Association, 10th April, 1947.

can draw no conclusion. We are also assuming that p is necessarily true or false, for if there were a third possibility over and above the truth and falsehood of p, then rejecting *not-p* would not lead to p because the third possibility would remain open.

These two assumptions, that a proposition cannot be both true and false, and that any proposition is *either* true *or* false, are called the laws of *contradiction* and *excluded middle* or *tertium non datur*. Unless these laws are established we are unable to justify the method of *reductio ad absurdum*, and in fact the whole weight of the attack which has been made on indirect proof in the past thirty years has fallen upon the law of excluded middle.

No one has yet succeeded in drawing a blank contradiction from an appeal to the *tertium non datur*. If that had happened there would be no controversy about the validity of indirect proof, because all mathematicians would reject the method. What the opponents of indirect proof have been concerned to show is that *reductio ad absurdum* leads to *absurd* rather than contradictory results. And, of course, what is, or is not, absurd is a matter of opinion.

The crux of the argument is the notion of existence in mathematics. By applying the law of contradiction and the *tertium non datur* to an existential proposition, the existence of a number with some specified property, or a function, or a set, is established by showing that non-existence leads to a contradiction, and this indirect method of proving existence is very widely used in mathematics in the so-called *pure existence theorems*. In elementary analysis, one of the most familiar of these pure existence theorems is the theorem which says that a non-decreasing bounded sequence converges to a unique limit; let us apply this theorem to a numerical sequence a_n, $n \geqslant 1$, defined as follows:

$a_n = 0$, if every even number from 2 to $2n$ is a sum of two primes,
$= 1$ otherwise.

Since we can work out whether any given even number is, or is not, a sum of two primes (granted enough time, patience and paper), we can work out the value of a_n for any given n, and so the sequence is definite. In fact up to any r that has so far been tried, $2r$ is a sum of two primes, so that $a_r = 0$.

Since each term takes either the value zero or unity, the sequence is bounded, and it is non-decreasing because, if $a_n = 1$ for some n, then one of the even numbers from 2 to $2n$ is not a sum of two primes, and so $a_{n+1} = 1$. Our existence theorem tells us, therefore, that a_n has a unique limit. What is it? The sequence commences with a run of zeros; if every term is zero, then the limit is zero, but if one term takes the value unity, then so do the remainder, and the limit is unity. That is to say, if Goldbach's hypothesis that every even number is a sum of two primes, is true, then the limit is zero, and if the hypothesis is false then the limit is unity. Since we do not know whether Goldbach's hypothesis is true or not, we do not know which of the numbers zero or unity *is* the limit of the sequence, or for that matter, that either of them is a limit. On the one hand we have a proof that the limit exists, and on the other we are faced with the fact that we have no way of finding it. The sequence is perfectly definite, but the limit is not; and if we call the limit supplied by the existence theorem a *number* l, then the position of l in the number series is indeterminate: for instance, we do not know if $l = 0$ or if $l > 0$. Now this is not a formal contradiction, for we have not proved that there is not a unique limit, but I suggest that, since it is impossible to find the limit, to say that we have proved there is a limit, is absurd.

Let me anticipate an obvious objection. It might be said: Surely we do know what the limit is; it is zero if Goldbach's hypothesis is true, and unity if the hypothesis is false. But I do not think this will do. In the first place

the values of the terms of the sequence do not depend upon proving Goldbach's hypothesis, so why should the limit? Moreover, Goldbach's problem may be insoluble; but even if we do find the limit by proving Goldbach's hypothesis, it would not alter the fact that the existence theorem we started from did *not* determine the limit.

The existence theorem really adds nothing whatever to our knowledge of the sequence a_n.

We could express the conclusion we have reached by saying that propositions about sequences, for instance the proposition that a sequence s_n has a limit l, admit a third possibility over and above their truth and falsehood. Such propositions may be true, false or indeterminate (insoluble).

There is nothing mysterious about this idea of a third possibility, and it is quite easy to find "illustrations" outside mathematics. Almost every day the newspapers tabulate the replies they receive to some question, in the form:

Yes - - -	37%
No - - -	60%
Don't know - -	3%

The law courts provide another example. English law applies the *tertium non datur*, for it holds a man innocent if he is not proved guilty, but Scottish law admits three verdicts, innocent, guilty and not-proven, so that in Scotland a refutation of guilt is not a proof of innocence. You can learn a lot about the foundations of mathematics by studying the law.

One of the fundamental differences between direct and indirect proof is that the former does not *presuppose* that the result we are seeking to prove makes sense. The direct proof supplies a meaning by joining the conclusion on to the general body of mathematical knowledge; whereas, in an indirect proof, we start by assuming that the conclusion is a significant proposition, one to which the law of excluded middle may be applied. This is shown rather strikingly in the following example.

Let us prove $\sqrt{(-1)} \leqslant 0$, by *reductio ad absurdum*.

If $\sqrt{(-1)} > 0$, then $\sqrt{(-1)} \times \sqrt{(-1)} > 0$, *i.e.* $-1 > 0$, which is false, and so $\sqrt{(-1)} \leqslant 0$. We all know this conclusion is nonsense, but what is wrong with the proof? Of course, we could proceed to prove also the contrary, that $\sqrt{(-1)} > 0$; for if $\sqrt{(-1)} \leqslant 0$ then $-\sqrt{(-1)} \geqslant 0$ and so $\{-\sqrt{(-1)}\}^2 \geqslant 0$, *i.e.* $-1 \geqslant 0$, which again is false. Do we conclude that mathematics is self-contradictory?

Just as the unsuccessful attempts to prove the parallel axiom culminated a century earlier in the discovery of non-Euclidean geometry, so the dispute over the validity of the *tertium non datur* led to many interesting and important discoveries in mathematics and symbolic logic.

The attack on *reductio ad absurdum* was started in 1913 by the Dutch mathematician L. E. J. Brouwer. At that time, Brouwer, who had already won a world-wide reputation by his work on topology, was one of the editors of the *Mathematische Annalen*, and he opened the attack by rejecting all papers offered to the *Annalen* which applied the *tertium non datur* to propositions the truth or falsehood of which could not be decided in a finite number of steps. The Editorial Board met this emergency by resigning—and then re-electing themselves, minus Brouwer. Incidentally, the Dutch government so resented this slight on their leading mathematician that they founded a rival mathematical journal, with Brouwer in charge.

The defence of *reductio ad absurdum* was undertaken by Hilbert, who regarded Brouwer's criticisms as a personal affront. Hilbert planned to dispel

all doubts of the validity of the indirect method, by PROVING that classical mathematics is free from contradiction, and contains no insoluble problems; this proof of course using only arguments accepted by everyone.

The formalist programme—as this ambitious plan was called—was abandoned in 1931 following the publication of Gödel's remarkable proof that every sufficiently rich mathematical system contains problems which are demonstrably insoluble. Gödel also established the impossibility of a proof of freedom from contradiction within the framework of classical mathematics. The question was re-opened, however, some five years later when Gentzen succeeded in proving that classical number theory is free from contradiction, but his proof transcended the limitations laid down in the formalist programme. Quite recently a system of mathematics has been constructed which is totally independent of the axioms of logic.

Another important consequence of the dispute was the discovery in 1920–21, independently by Emil Post in America and Lucasiewicz in Poland, of a multi-valued formal logic which rejects the *tertium non datur*. The discovery proved to be a fruitful one, and during the past ten years three-valued logics have been applied in such diverse fields as the foundations of mathematics, and quantum physics, in the latter case with striking success.

A logic is specified by its fundamental matrices (which is another way of saying it is an exercise in permutations and combinations). Writing T and F for true and false, respectively, T being considered the higher of the two values, the matrices for the classical two-valued logic are as follows:

p	*not-p*
T	F
F	T

p	q	*p and q*	*p or q*	*p implies q*	*not-p*	*p or not-p*
T	T	T	T	T	F	T
T	F	F	T	F	F	T
F	T	F	T	T	T	T
F	F	F	F	T	T	T

The first two columns simply exhibit the four ways of combining T and F. The third column defines "*p and q*" as the function of p, q whose value is the *lower* of the values of p and q, and similarly the fourth column picks out the *higher* value to define "*p or q*". There are sixteen different ways of correlating a column of T's and F's with the first two columns, that is to say, sixteen functions of p, q; six of these are p, q, *not-p*, *not-q*, *p and q*, *p or q*, and it can easily be verified that the remaining ten are expressible in terms of these six (in fact, just the three functions p, q, *neither p nor q* suffice). For instance the function with values $TFFT$ (in order) is

(*p and q*) *or* (*not-p and not-q*).

Note that *not-not-p* has the same values as p, and (*p implies q*) the same as (*not-p or q*). If you work out the values of the function *p or not-p* you find all T's; in other words (*p or not-p*) is true irrespective of whether p is true or false. It is not necessary for this proof of (*p or not-p*) that p be just an elementary proposition; the proof applies when p is any *finite* combination of elementary propositions, but it does *not* apply to an infinite set of propositions (like *every even number is a sum of two primes*). Thus the *tertium non datur* is demonstrable for propositions which have just two values. Accordingly any dispute about the *tertium non datur* is necessarily a dispute about the number of truth values of a proposition.

The corresponding matrices for a logic with three values true, indeterminate, false (in descending order) are

p	*not-p*
T	I
I	F
F	T

p	q	*p and q*	*p or q*
T	T	T	T
T	I	I	T
T	F	F	T
I	T	I	T
I	I	I	I
I	F	F	I
F	T	F	T
F	I	F	I
F	F	F	F
		lower value	higher value

Here *not-not-p* differs from p, but *not-not-not-p* has the same value as p ; and instead of a *tertium non datur* we have a *quartum non datur* :

$$p \text{ or } (not\text{-}p) \text{ or } (not\text{-}not\text{-}p).$$

I should like to suggest now a rather different way of looking at the whole question of indirect proof. Instead of asking whether *reductio ad absurdum* is a valid method of proof, let us examine instead how much *information* such a proof gives us. We shall find that the answer varies from case to case, from no information at all at one end to all we could desire at the other. I shall try to illustrate this in the following examples.

1. As a first instance I shall take the well-known inequality :

$$\text{if } a, b \text{ are real and positive, } \quad \tfrac{1}{2}(a+b) \geqslant \sqrt{(ab)}.$$

By *reductio ad absurdum* we argue as follows : From $\frac{1}{2}(a+b) < \sqrt{(ab)}$ we derive in turn $\{\frac{1}{2}(a+b)\}^2 < ab$, $a^2 - 2ab + b^2 < 0$, $(a-b)^2 < 0$, which contradicts the hypothesis that a and b are real. If we write this *reductio ad absurdum* proof *backwards*, simply replacing $<$ by $\geqslant$ we obtain a direct proof of the inequality, so that the direct and indirect proofs are simply journeys to and fro along the same road. As there can be no special merit in proceeding one way rather than the other, in this case direct and indirect proof are equally successful, and give the *same* information.

2. *The infinity of primes.* Here is the current *reductio ad absurdum* proof. Let N be the greatest prime. Since $N!+1$ is not divisible by any of the numbers 2 to N, its least factor (above unity) must be a prime greater than N, which contradicts the hypothesis that N is the greatest prime. Compare this with a direct proof. Make no assumption about N and consider $N!+1$; the least factor of this number is a prime greater than N. Hence if $N_0 = 2$ and N_{r+1} is the least factor (above unity) of $N_r! + 1$ for all r, then N_0, N_1, N_2, ... is an increasing infinite sequence of primes, and there is no greatest prime.

The direct and the indirect proof give the same information, namely the formula " the least factor of $N!+1$ " for a prime greater than any N. My objection to the indirect proof in this case is on grounds of elegance. The indirect proof makes a quite superfluous assumption.

3. *The irrationality of* $\sqrt{2}$. I need not reproduce the details of the classical *reductio ad absurdum* proof. It will suffice to mention that from the assumption that there is a fraction p/q, where p, q are relatively prime, such that $(p/q)^2 = 2$, we deduce that p, q have a common factor 2 contrary to the hypothesis.

How much information is contained in this *reductio ad absurdum?* Does the proof tell us, for instance, by how much $\sqrt{2}$ differs from p/q? It does not ;

and if the proof proves $\sqrt{2}$ is irrational, then it proves, presumably, that in the expansion of $\sqrt{2}$ a digit cannot recur infinitely often. Well, how many times can a digit recur in this expansion? Again the proof is silent. In fact, the proof gives no information whatever other than that the assumption $(p/q)^2 = 2$ leads to a contradiction.

The surprising thing is that a direct proof gives all this information without adding one iota to the amount of work we have to do.

For considering odd and even values of p, q we see that $p^2 - 2q^2$ is a product of an odd number with a power of 2, and so

$$| p^2 - 2q^2 | \geqslant 1,$$

whence

$$| p^2/q^2 - 2 | \geqslant 1/q^2. \quad \text{.....(i)}$$

This is a direct proof that 2 is not the square of a rational number, which at the same time sets a lower bound to the difference. Let x_n be the expansion of $\sqrt{2}$ to n places of decimals, so that

$$x_n{}^2 < 2 < (x_n + 1/10^n)^2 ;$$

then from inequality (i) we readily derive

$$| x_n - p/q | \geqslant 1/6 \, | pq |, \quad \text{.....(ii)}$$

provided that n exceeds the number of digits in q^2.

If, further, $x_n = 1 \,.\, a_1 a_2 \ldots a_{n-1} \alpha\alpha \ldots \alpha a_{N+1} a_{N+2} \ldots$, and

$$x = 1 \,.\, a_1 a_2 \ldots a_{n-1} \alpha = (9i + \alpha)/9 \,.\, 10^{n-1} = p/q, \text{ say,}$$

where i, p, q are integers, then $| x - x_m | < 1/10^n$, for $m \geqslant N$; but

$$| x - x_m | \geqslant 1/6 \, | p/q \, | \, q^2 = 1/6x \,.\, 9^2 \,.\, 10^{2n-2} > 1/9^2 \,.\, 10^{2n-2} > 1/10^{2n+1}.$$

Hence $N < 2n + 1$, and so a recurrence in the expansion of $\sqrt{2}$ which commences at the nth digit terminates not later than the $(2n)$th.

For the sake of completeness I should like to give also a direct proof of the trivial theorem with which I opened this lecture. We have to prove that if $\frac{1}{2}\{1 + (-1)^n\}$ is odd, then n is even. Let $R(p)$ be the remainder when p is divided by 2; then, considering odd and even values of p,

$$R(p) \,.\, R(\tfrac{1}{2}\{1 + (-1)^p\}) = 0, \text{ for all } p. \quad \text{.....(i)}$$

Since $R(\frac{1}{2}\{1 + (-1)^n\}) = 1$, therefore $R(n) = 0$, and so n is even.

This of course is a proof by cases, but it may readily be transformed into a pure inductive proof as follows.

We define $\sigma(0) = 1$, $\sigma(p + 1) = 0$; $R(0) = 0$, $R(p + 1) = \sigma(R(p))$, then

$$R(p + 2) = R(p), \quad \text{.....(ii)}$$

for $R(2) = \sigma(\sigma(R(0))) = \sigma(1) = 0 = R(0)$, so that (ii) holds for $p = 0$, and if (ii) holds for $p = k$, then $R(k + 3) = \sigma(R(k + 2)) = \sigma(R(k)) = R(k + 1)$, which completes the proof of equation (ii). Let $\phi(p)$ denote $R(\frac{1}{2}\{1 + (-1)^p\})$, then we have $R(0) = 0, R(1) = 1, R(p + 2) = R(p)$ and $\phi(0) = 1, \phi(1) = 0, \phi(p + 2) = \phi(p)$. Hence if $\delta(p) = | \, \phi(p) R(p) - \phi(p + 1) R(p + 1) \, |$, then $\delta(0) = 0$, and $\delta(p + 1) = \delta(p)$, so that $\delta(p) = 0$ for all p, that is, $\phi(p) R(p) = \phi(p + 1) R(p + 1)$; but $\phi(0) R(0) = 0$, and so $\phi(p) R(p) = 0$ for all p, which proves equation (i). It remains to show that if $R(n) = 0$ then n is even. If $W(p)$ is defined by the equations

$$W(0) = 0, \quad W(p + 1) = W(p) + R(p),$$

then, observing that $R(p) + R(p + 1) = 1$, a simple induction proves

$$p = 2W(p) + R(p);$$

hence if $R(n) = 0$ then $n = 2W(n)$, and so n is even.

One last observation. Let us apply a *reductio ad absurdum* argument to the unsolved Goldbach's problem.

It is evident that if we could find a number k so that every even number is a sum of two primes provided only that the first k even numbers are sums of two primes, then the problem is solved. We have only to test the first k even numbers to find out whether Goldbach's hypothesis is true or not.

Now to the proof that this number k exists. Consider the sequence a_n we introduced before :

$a_n = 0$ if $2r$ is a sum of two primes for all r from 1 to n,
$= 1$, otherwise.

The sequence a_n is bounded and non-decreasing, and so convergent.

Hence there is a k such that

$$a_n - a_k < 1/2 \text{ for all } n > k.$$

If for this k, $a_k = 0$, then all a_n are zero, and Goldbach's hypothesis is true; if $a_k = 1$ then the hypothesis is false. For a given k we can work out the value of a_k ; I leave it to you just to test whether this a_k is 0 or 1. Of course, the same argument may be applied to any other unsolved problem in the theory of numbers—for instance, to Fermat's last problem.

How much information does this proof give you?

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R. L. G.

1579. (54). The student must here avoid the mistake which . . . I have twice fallen into in the course of this work.—De Morgan, *Differential and Integral Calculus*, 372.

1580. (200). Besides, if $\frac{4}{\frac{2}{3}}$ were a fraction, the denominator $\frac{2}{3}$ would indicate that some unit had been divided into $\frac{2}{3}$ equal parts! Since it is impossible for *man* to so divide a unit, this species of complex fractions must be regarded as a special gift from on high, " for with God all things are possible ". (From an article by the Principal of a school, in the . . . . . , 1894.)

MATHEMATICAL NOTES

2017. *A new proof of two theorems on the multiplication of determinants.*

The following proof of the rule for the multiplication of determinants appears to possess two advantages over the proofs given in most textbooks: it is more direct, and it yields another well-known theorem at the same time.

Adopting the Einstein summation convention, we begin by considering two systems of linear equations,

$$a_{rs}x_r = y_s, \quad b_{st}y_s = z_t, \qquad \text{.......(1)}$$

where $r, s, t = 1, 2 \ldots n$. Combining them, we obtain

$$c_{rt}x_r = z_t, \qquad \text{.......(2)}$$

where

$$c_{rt} = a_{rs}b_{st}. \qquad \text{.......(3)}$$

Solving equations (1) for x in terms of y and for y in terms of z, we get

$$A_{rs}y_s = D(a)\,x_r, \quad B_{st}z_t = D(b)\,y_s, \qquad \text{.......(4)}$$

where A_{rs} is the cofactor of a_{rs} in the determinant, $D(a) = \| a_{rs} \|$, and B_{st} is the cofactor of b_{st} in the determinant, $D(b) = \| b_{st} \|$. Combining equations (4), we find that

$$A_{rs}B_{st}z_t = D(a)\,D(b)\,x_r. \qquad \text{.......(5)}$$

If, however, we solve equation (2) for x in terms of z we obtain

$$C_{rt}z_t = D(c)\,x_r, \qquad \text{.......(6)}$$

where C_{rt} is the cofactor of c_{rt} in the determinant, $D(c) = \| c_{rt} \|$. Comparison of (5) and (6) shows that

$$A_{rs}B_{st}D(c) = C_{rt}D(a)D(b). \qquad \text{.......(7)}$$

If a_{rs} is replaced by b_{sr} and b_{st} by a_{ts}, for all r, s and t, we see from (3) that c_{rt} is replaced by c_{tr}. Hence, according as $\| a_{rs} \|$ is, or is not, a factor of $\| c_{rt} \|$, so $\| b_{sr} \|$ is, or is not, a factor of $\| c_{tr} \|$; *i.e.* according as $D(a)$ is or is not a factor of $D(c)$, so $D(b)$ is or is not a factor of $D(c)$. Moreover, since all the elements a_{rs} and b_{st} are arbitrary, in general $D(a)$ and $D(b)$ will have no factors. Consequently, it follows from (7) that $D(a)\,D(b)$ must be either a factor of $D(c)$ or else of $A_{rs}B_{st}$. But the degree of $A_{rs}B_{st}$ in the elements a_{rs} and b_{st} is only $2(n-1)$, while that of $D(a)\,D(b)$ is $2n$. Thus $D(a)\,D(b)$ must be a factor of $D(c)$, and since $D(c)$ is of degree $2n$ in the elements a_{rs} and b_{st}, we must have

$$D(c) = kD(a)\,D(b), \qquad \text{.......(8)}$$

where k is the same for all values of the elements a_{rs} and b_{st}. By taking a_{rs} and b_{st} as the corresponding Kronecker deltas, δ_{rs} and δ_{st} respectively, we immediately deduce that k is unity. We have thus proved that

$$D(c) = D(a)\,D(b), \qquad \text{.......(9)}$$

and also that

$$C_{rt} = A_{rs}B_{st}, \qquad \text{.......(10)}$$

which is the analogue of (3).

G. J. Whitrow.

2018. *On Note* 1940 ($\sqrt{a}$).

I must concede to Mr. Swinden that the convention making $\sqrt{a}$ positive is not compulsory. But it does seem convenient in the context of *Gazette*, XXX, p. 3, and in other places involving integrals containing square roots.

The paradox $x^2 = x^{4/2} = \pm\sqrt{x^4}$ is obtained by using Mr. Swinden's convention for the first step and mine for the second. It shows that both conventions must not be used at once.

Another point is raised by the paradox. It does not follow from $a = b$ that everything that is true about a is true about b. For instance, c^a need not be the same as c^b. Thus it is not to be lightly assumed that x^2 is the same as $x^{4 \cdot \frac{1}{2}}$, whether it is the same or not depends upon the meaning that has been given by definition to $x^{p/q}$. Or, again, $\frac{1}{2} = \frac{2}{4}$, but if z is a complex number and if the usual convention is adopted $z^{1/2}$ is not the same as $z^{2/4}$, because one of them has 2 values and the other 4; and quite apart from complex numbers, although $\frac{1}{2} = \frac{2}{4}$, nobody would say that therefore the denominator of $\frac{1}{2}$ is equal to the denominator of $\frac{2}{4}$.

Different uses of " = " are notoriously the source of a good deal of confusion, notably the confusion of equality and identity in ordinary algebra and in coordinate geometry. A. R.

2019. *On Note* **1954.**

The polynomial of degree n which takes the value a when $x = x_0$, and b when $x = x_1, x_2, \ldots, x_n$, can be written down immediately as

$$p(x) = a + (b - a)\frac{(x - x_1)(x - x_2) \ldots (x - x_n)}{(x_0 - x_1)(x_0 - x_2) \ldots (x_0 - x_n)},$$

so that

$$p(0) = a + (b - a)\frac{x_1 x_2 \ldots x_n}{(x_1 - x_0)(x_2 - x_0) \ldots (x_n - x_0)}$$
$$= a + (b - a)c, \text{ say.}$$

All that is now needed is some information about c, and this (with much that is irrelevant) is provided by the equation connecting the x's.

R. D. LORD.

2020. *On Note* **1999**: *The projection of a conic into a circle.*

The tangent cone to a conicoid from a point outside it is a cone of revolution if and only if the point is a focus of the conicoid. Any conic C determines uniquely a confocal system of conicoids, and this system includes one other conic D as a limiting form. The right circular cones of which C is a section are the cones through C whose vertices are on D.

If this argument is thought to belong to too advanced a field, an appeal to familiar elementary theorems will be preferred; after all, the result to be established is metrical, not projective.

If the normal, at a point P of a conic D, cuts the axes in G, g, then

$$PG = (1 - e^2)\,Pg,$$

and therefore

$$PG = (e_1^2 - 1)\,Gg,$$

where $e_1 = 1/e$. Since the four points S, S', P, g are concyclic,

$$PG^2 = (e_1^2 - 1)\,SG \,.\, GS'.$$

Now let the tangent at P cut the focal axis in T, and let the line through T at right angles to PT cut SP in U. Then

$$TU^2/ST \,.\, TS' = -PG^2/SG \,.\, GS' = 1 - e_1^2.$$

Thus TU^2 is equal to the square of the ordinate through T of any conic C which has S, S' for vertices and $1/e$ for eccentricity, that is, which has S, S' for vertices and A, A' for foci. If the plane of C is perpendicular to the plane of D, then C is a section of a right circular cone with PT for axis.

E. H. N.

2021. *Division of angles.*

It can be shown that :

$$(a+bi)=(c+di)\{(ac+bd)+(bc-ad)i\}/(c^2+d^2).$$

Thus : Angle $(a+bi)$ = Angle $(c+di)$ + Angle $\{(ac+bd)+(bc-ad)i\}$.

Having arrived at this result, one or two examples will better explain the method.

Example I. Find $\frac{1}{3}$ Angle $(5+2i)$.

Angle $(5+2i)$ = Angle $(8+i)$ + Angle $(42+11i)$,

Angle $(42+11i)$ = Angle $(8+i)$ + Angle $(347+46i)$,

Angle $(347+46i)$ = Angle $(8+i)$ + Angle $(2822+21i)$,

Angle $(2822+21i)$ = approx. $3\times$ Angle $(2822+7i)$.

Hence, $\frac{1}{3}$ Angle $(5+2i)$ = approx. Angle $(8+i)$ + Angle $(2822+7i)$,

= approx. Angle $(22569+2878i)$.

Example II. Find $\frac{1}{4}$ Angle $(-1+i)$ or 135°.

Angle $(-1+i)$ = Angle $(3+2i)$ + Angle $(-1+5i)$,

Angle $(-1+5i)$ = Angle $(3+2i)$ + Angle $(7+17i)$,

Angle $(7+17i)$ = Angle $(3+2i)$ + Angle $(55+37i)$,

Angle $(55+37i)$ = Angle $(3+2i)$ + Angle $(239+i)$,

Angle $(239+i)$ = approx. $4\times$ Angle $(956+i)$.

Thus, $\frac{1}{4}$ Angle $(-1+i)$ = approx. Angle $(3+2i)$ + Angle $(956+i)$,

= approx. Angle $(2866+1915i)$,

and hence $\text{Tan } 33\frac{3}{4}^\circ=\frac{1915}{2866}=\cdot 6681786$, correct to 7 d.c.

It will be noticed that the size of the angle is immaterial. In fact, we could operate on multiples of π if required.

Also note that the convergence at each step is extremely high.

R. H. Birch.

2022. *The heptasphen.*

The term polyhedron focusses attention on the number of faces of the solid to which it refers ; thus a tetrahedron has four faces, a pentahedron five faces, and so on. It is sometimes desirable, as for example when applying the principle of duality, to lay stress on the number of vertices of the solid. In such circumstances the term *polycorypha* (due to Sommerville) or *polyacron* (due to Cayley) may be used. Adopting Sommerville's nomenclature, we should then have tetracorypha, pentacorypha, and so on.

If we should wish to lay the emphasis on the number of edges of the solid, then the term " polysphen " might be used. The least number of edges of any solid being six, we have then : the hexasphen, the heptasphen, the octosphen, and so on. In this series of polysphens the hexasphen is the familiar tetrahedron and need not detain us further ; the heptasphen, however, is of special interest. It is unique in the series. It does not exist!

This is readily proved but is not too obvious, so that the heptasphen has a plausibility denied to " square circles ", " married bachelors ", etc., which are usually used as illustrations of the null-class in logic. The hidden inconsistency in the definition of the heptasphen brings home the need for proving " existence theorems " in mathematics.

A. G. Davies.

2023. *An apparent difficulty.*

The usual proof that

$$\frac{c_1}{1}-\frac{c_2}{2}+\frac{c_3}{3}\ldots+(-)^{n-1}\frac{c_n}{n}=1+\tfrac{1}{2}+\ldots+\frac{1}{n},$$

where c_r denotes ${}^nc_r$, is by induction, using the result

$${}^nc_r={}^{n-1}c_r+{}^{n-1}c_{r-1}.$$

The following method, using partial fractions, is not usually used for this particular series because of the apparent difficulty of a zero factor occurring in the denominator. But the difficulty can be got over easily as indicated below.

By splitting into partial fractions, we have

$$\frac{n!}{x(x+1)\ldots(x+n)}=\frac{1}{x}-\frac{c_1}{x+1}+\ldots+\frac{(-)^n c_n}{x+n}.$$

Direct substitution of $x=0$ at this stage is useless. But there is no difficulty if we first make a slight simplification. We get

$$\begin{aligned}\frac{c_1}{x+1}-\frac{c_2}{x+2}+\ldots+\frac{(-)^{n-1}c_n}{x+n}&=\frac{1}{x}\left\{1-\frac{n!}{(x+1)\ldots(x+n)}\right\}\\&=\frac{(x+1)\ldots(x+n)-n!}{x(x+1)\ldots(x+n)}\\&=\frac{S_{n-1}x+S_{n-2}x^2+\ldots+x^n}{x(x+1)\ldots(x+n)}\\&=\frac{S_{n-1}+S_{n-2}x+\ldots+x^{n-1}}{(x+1)(x+2)\ldots(x+n)},\end{aligned}$$

where S_r is the sum of the products of the first n natural numbers taken r at a time. Now when $x=0$,

$$\frac{c_1}{1}-\frac{c_2}{2}+\ldots+(-)^{n-1}\frac{c_n}{n}=\frac{S_{n-1}}{n!}=1+\tfrac{1}{2}+\tfrac{1}{3}+\ldots+\frac{1}{n}.$$

I must thank Mr. C. S. Pillai for asking me for a non-inductive proof of this result.

S. PARAMESWARAN.

2024. *Note on prime groups.*

In G. H. Hardy and E. M. Wright, *An Introduction to the Theory of Numbers*, reference is made to prime-pairs $(p,\ p+2)$ and prime-triplets $(p,\ p+2,\ p+6)$, $(p,\ p+4,\ p+6)$ followed by the conjecture that the number of such groups is infinite.

Having prepared a table of the complete factors of 18000 numbers following 10^9, I thought it might be interesting to search for any such groups contained in it. The result is given below.

Cases of $p,\ p+2,\ p+6$, occur for $p=10000\ 02821,\ 10000\ 04891$.

Cases of $p,\ p+4,\ p+6$ occur for $p=10000\ 03267,\ 10000\ 07647,\ 10000\ 07921,\ 10000\ 13137$.

There are also two cases of quadruplets, $p.\ p+4,\ p+6,\ p+10$, given by $p=10000\ 11763$ and $p=10000\ 16617$.

There are no cases of $p,\ p+2,\ p+6,\ p+8$.

The attached table gives a comparison of the frequency of such groups at different levels, though they occur so irregularly that one set of numbers at each level may not be sufficient. In each case the groups noted are contained in 10000 numbers immediately following the number in the first column.

ONE OF THE BAYS IN THE CASEROOM OF ROBERT MACLEHOSE & COMPANY, LIMITED, THE UNIVERSITY PRESS, GLASGOW, SHOWING A SECTION OF MATHEMATICAL COMPOSITORS AT WORK IN 1948

		Number of prime groups			
	Primes	2	3	4	5
10^5	860	60	30	3	1
10^7	614	42	11	2	0
$2 \cdot 10^7$	595	33	10	2	0
$3 \cdot 10^7$	597	23	8	0	0
10^9	487	25	5	0	0

E. S. DAVIS.

2025. *Note on the circular pendulum.*

The following theorem admits of very elementary proof: If a particle rotates in a vertical circle under gravity, the join of two points separated by a half-period passes through a fixed point.

Let P, with abscissa PL, be any point on the circle

$$(x-c)^2+y^2=2bx,$$

whose centre is B. The circle is one of a coaxial family whose radical axis is OY, a real limiting point being C at $(c, 0)$. The equation states that

$$PC^2=2BC \cdot PL.$$

If PC meet the circle again in Q with abscissa QM, we have

$$PL : QM = PC^2 : QC^2.$$

For an infinitesimal displacement to $P'CQ'$, we have

$$PP' : QQ' = PC : QC.$$

Turn the figure clockwise through a right angle. If the velocity at P is due to a fall from LM, the same applies at Q. Hence

$$\text{velocity at } P : \text{velocity at } Q = PL^{\frac{1}{2}} : QM^{\frac{1}{2}} = PC : QC = PP' : QQ'.$$

Hence PP' and QQ' are described in equal times, and the same applies to PQ and $P'Q'$. The time from P to Q is therefore independent of the position of P; and the vertical position of PQ gives the time as half the period of rotation.

F. E. RELTON.

2026. *On Note* 1984 (XXXI, October, 1947).

In this Note Mr. Ehrenberg proves the theorem:

If $f''(x)>0$ (<0) in the interval (a, b), if $x_1, x_2, \ldots, x_n$ all lie in the interval (a, b) and if $x_1+x_2+\ldots+x_n=c$, where c is constant, then $f(x_1)+f(x_2)+\ldots+f(x_n)$ is a minimum (maximum) when $x_1=x_2=\ldots=x_n=c/n$.

He does this by replacing any two of the x_r which are not equal by their arithmetic mean and this decreases (increases) $\Sigma f(x_r)$. He then says "Hence $\Sigma f(x_r)$ is least (greatest) when $x_1=x_2=\ldots=x_n=c/n$". In doing this, he assumes the existence of a set of the x_r in which $\Sigma f(x_r)$ attains its minimum (maximum) value. The existence of the set can be proved by considering the continuity of the function

$$f(x_1)+f(x_2)+\ldots+f(x_{n-1})+f(c-x_1-\ldots-x_{n-1}),$$

when the $n-1$ variables $x_1, \ldots, x_{n-1}$ lie in (a, b), but such considerations are hardly suitable for the sixth form boy for whom the proof is intended. The following proof avoids this difficulty and is equally simple.

Let us suppose that $f''(x)>0$, so that the curve is concave up. (If $f''(x)<0$,

all inequalities are reversed.) Since the curve lies below the chord joining the points with abscissae x_1, x_2, if $p>0$, $q>0$, then

$$f\left(\frac{px_1+qx_2}{p+q}\right)<\left(\frac{pf(x_1)+qf(x_2)}{p+q}\right),$$

and

$$f\left(\frac{qx_1+px_2}{p+q}\right)<\left(\frac{qf(x_1)+pf(x_2)}{p+q}\right).$$

Hence

$$f\left(\frac{px_1+qx_2}{p+q}\right)+f\left(\frac{qx_1+px_2}{p+q}\right)<f(x_1)+f(x_2).$$

If all the x_r are not equal, let x_1 be the greatest and x_2 the least of them, so that

$$x_1>c/n>x_2.$$

Replace x_1 by c/n and x_2 by x_1+x_2-c/n, so that the sum is unchanged. If c/n divides (x_1, x_2) in the ratio $q:p$, then

$$\frac{c}{n}=\frac{px_1+qx_2}{p+q}, \quad x_1+x_2-\frac{c}{n}=\frac{qx_1+px_2}{p+q},$$

and therefore

$$f(c/n)+f(x_1+x_2-c/n)<f(x_1)+f(x_2),$$

so $\Sigma f(x_r)$ is decreased. Repeating the process we see that in at most $n-1$ steps every x_r will have been replaced by c/n. At each step $\Sigma f(x_r)$ is decreased and its minimum value is therefore given by $x_1=x_2=\ldots=x_n=c/n$.

IDA W. BUSBRIDGE.

2027. *Maxima and minima and trigonometrical inequalities.*

1. Professor Brown's article on "Elementary inequalities" in the *Mathematical Gazette*, February, 1941, contains several interesting applications of inequalities even at the very foundations of the subject. It is the object of this note to show that the theorem of the means, viz. that the arithmetic mean of n positive numbers is not less than the geometric mean, may be used to prove theorems on maxima and minima trigonometrically.

2. *Theorem I. The triangle of greatest area or perimeter inscribed in a given circle is equilateral.*

Trigonometrically, this is equivalent to

(i) $$\sin A \sin B \sin C \leqslant \frac{3\sqrt{3}}{8}$$

(ii) $$\sin A+\sin B+\sin C \leqslant \frac{3\sqrt{3}}{2}$$

in any triangle ABC.

Case (i). Let A, B, C be all acute. Then $\cos A$, $\cos B$, $\cos C$ are all positive. Since

$$\cos^2 A+\cos^2 B+\cos^2 C+2\cos A\cos B\cos C=1, \quad \ldots\ldots\ldots\ldots(1)$$

we have from the theorem of the means, if A, B, C are all acute angles,

$$\sqrt[4]{\{\cos^2 A\cos^2 B\cos^2 C(2\cos A\cos B\cos C)\}}\leqslant\tfrac{1}{4},$$

whence

$$\cos A\cos B\cos C\leqslant\tfrac{1}{8}. \quad \ldots\ldots\ldots\ldots(2)$$

Further, from the same identity (1), and under the same conditions for A, B, and C, we obtain, using (2),

$$\sin^2 A+\sin^2 B+\sin^2 C=3-(1-2\cos A\cos B\cos C)\leqslant\tfrac{9}{4}.$$

Again, applying the theorem of the means,

$$\sqrt[3]{(\sin^2 A \sin^2 B \sin^2 C)} \leqslant \tfrac{1}{3}(\sin^2 A + \sin^2 B + \sin^2 C) \leqslant \tfrac{3}{4};$$

thus
$$\sin A \sin B \sin C \leqslant 3\sqrt{3}/8, \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(3)$$

where $\sin A$, $\sin B$, $\sin C$ are all positive.

Case (ii). Let A be obtuse or right. Then B and C are both acute, so that $\sin B \leqslant \cos C$, and $\sin B \sin C \leqslant \sin C \cos C = \frac{1}{2} \sin 2C \leqslant \frac{1}{2}$. Since $\frac{1}{2} < 3\sqrt{3}/8$ and $\sin A \leqslant 1$, we have, even in this case,

$$\sin A \sin B \sin C \leqslant 3\sqrt{3}/8.$$

Again, applying the same inequality to the ex-central triangle of ABC, we have

$$\sin \tfrac{1}{2}(\pi - A) \sin \tfrac{1}{2}(\pi - B) \sin \tfrac{1}{2}(\pi - C) \leqslant 3\sqrt{3}/8,$$

that is,
$$\cos \tfrac{1}{2}A \cos \tfrac{1}{2}B \cos \tfrac{1}{2}C \leqslant 3\sqrt{3}/8.$$

Now
$$\sin A + \sin B + \sin C = 4 \cos \tfrac{1}{2}A \cos \tfrac{1}{2}B \cos \tfrac{1}{2}C,$$

so that
$$\sin A + \sin B + \sin C \leqslant 3\sqrt{3}/2.$$

The theorem follows if we recall the familiar formulae: if R is the circum-radius,

$$\triangle ABC = 2R^2 \sin A \sin B \sin C;$$
$$a + b + c = 2R(\sin A + \sin B + \sin C);$$

and the area and perimeter of an inscribed equilateral triangle are respectively $\frac{3}{4}\sqrt{3}R^2$ and $3\sqrt{3}R$.

3. *Theorem II. The triangle of least area or perimeter described * about a given circle is equilateral.*

Trigonometrically, this is equivalent to

$$\cot \tfrac{1}{2}A + \cot \tfrac{1}{2}B + \cot \tfrac{1}{2}C \geqslant 3\sqrt{3}.$$

Since
$$\tan \tfrac{1}{2}A \tan \tfrac{1}{2}B + \tan \tfrac{1}{2}B \tan \tfrac{1}{2}C + \tan \tfrac{1}{2}C \tan \tfrac{1}{2}A = 1,$$

we have from the theorem of the means applied to the positive numbers $\tan \frac{1}{2}A \tan \frac{1}{2}B$, $\tan \frac{1}{2}B \tan \frac{1}{2}C$, $\tan \frac{1}{2}C \tan \frac{1}{2}A$,

$$\sqrt[3]{(\tan^2 \tfrac{1}{2} A \tan^2 \tfrac{1}{2}B \tan^2 \tfrac{1}{2}C)} \leqslant \tfrac{1}{3},$$

whence
$$\tan \tfrac{1}{2}A \tan \tfrac{1}{2}B \tan \tfrac{1}{2}C \leqslant 1/3\sqrt{3},$$

or
$$\cot \tfrac{1}{2}A \cot \tfrac{1}{2}B \cot \tfrac{1}{2}C \geqslant 3\sqrt{3};$$

i.e.
$$\cot \tfrac{1}{2}A + \cot \tfrac{1}{2}B + \cot \tfrac{1}{2}C \geqslant 3\sqrt{3}.$$

More simply the same theorem of the means gives

$$\tfrac{1}{3}(\cot \tfrac{1}{2}A + \cot \tfrac{1}{2}B + \cot \tfrac{1}{2}C) \geqslant \sqrt[3]{(\cot \tfrac{1}{2}A \cot \tfrac{1}{2}B \cot \tfrac{1}{2}C)};$$

hence
$$\cot \tfrac{1}{2}A + \cot \tfrac{1}{2}B + \cot \tfrac{1}{2}C \geqslant 3\sqrt{3}.$$

If r is the radius of the given circle, it is readily seen that the perimeter and the area of any circumscribed triangle ABC are

$$2r(\cot \tfrac{1}{2}A + \cot \tfrac{1}{2}B + \cot \tfrac{1}{2}C)$$

and
$$r^2(\cot \tfrac{1}{2}A + \cot \tfrac{1}{2}B + \cot \tfrac{1}{2}C),$$

respectively, whence the theorem follows.

* It is understood here that the triangle is so drawn that the given circle becomes the in-circle and not one of the ex-circles.

4. *Theorem III. The maximum triangle inscribed within a given triangle and in perspective with it has its vertices at the middle points of the sides.*

Let ABC be a triangle, in which DEF is inscribed, such that D, E, F lie on BC, CA, AB respectively, and AD, BE, CF are concurrent. Let X, Y, Z be the middle points of BC, CA, AB respectively. Let $BD/DC = p$, $CE/EA = q$, and $AF/FB = r$, where $pqr = 1$, since AD, BE, CF are concurrent.

Then
$$\triangle CDE/\triangle CBA = q/(p+1)(q+1);$$
$$\triangle AEF/\triangle CBA = r/(q+1)(r+1);$$
$$\triangle BFD/\triangle CBA = p/(r+1)(p+1).$$

Hence
$$\frac{\triangle DFE}{\triangle CBA} = 1 - \frac{q}{(p+1)(q+1)} - \frac{r}{(q+1)(r+1)} - \frac{p}{(r+1)(p+1)}$$
$$= \frac{pqr+1}{(1+p)(1+q)(1+r)}$$
$$= \frac{2}{2+(p+q+r)+(pq+qr+rp)},$$
since $pqr = 1$.

Now, by the theorem of the means, we have, if p, q, r be all positive,
$$\tfrac{1}{3}(p+q+r) \geqslant \sqrt[3]{(pqr)} = 1,$$
that is,
$$p+q+r \geqslant 3;$$
and
$$\tfrac{1}{3}(pq+qr+rp) \geqslant \sqrt[3]{(p^2q^2r^2)} = 1,$$
whence
$$pq+qr+rp \geqslant 3.$$
Thus
$$\triangle DFE/\triangle CBA \leqslant \tfrac{1}{4} = \triangle XYZ/\triangle ABC,$$
that is,
$$\triangle DEF \leqslant \triangle XYZ.$$

Corollary. If $\triangle DEF = \triangle XYZ$, then the two triangles coincide; for
$$p+q+r+pq+qr+rp = 6,$$
and $pqr = 1$, imply $p = q = r = 1$, if $p > 0$, $q > 0$ and $r > 0$.

More explicitly, the condition may be reduced to
$$(p+p^{-1}-2)+(q+q^{-1}-2)+(r+r^{-1}-2) = 0,$$
since $pq = 1/r$, etc., and hence
$$(p-1)^2/p \quad + \quad (q-1)^2/q \quad + \quad (r-1)^2/r = 0.$$
Since each term is positive and the sum of the terms vanishes, each term vanishes. Thus we have $p = q = r = 1$.

A. A. KRISHNASWAMI AYYANGAR.

2028. *On the divergence of Σn^{-s}.*

It is usual to present the series $\Sigma(1/n)$ as the first and simplest example of a series of positive terms, tending monotonically to zero, whose sum is divergent. The proof is usually carried out by bracketing the terms in groups of 2, 4, 8, ... or by a comparison with the integral of $1/x$. This is so ingrained that one tends to prove the divergence of Σn^{-s} when $s < 1$ by a comparison with $\Sigma(1/n)$; but this is unnecessary. The following line of proof is quite free from grouping artifices.

Suppose that $0 < s < 1$; then
$$1^{-s} + 2^{-s} + \ldots + n^{-s} > n \,.\, n^{-s} = n^{1-s},$$
and n^{1-s} tends to infinity with n.

MAVIS K. HINDS.

REVIEWS.

Methods of Algebraic Geometry. I. By W. V. D. HODGE and D. PEDOE. Pp. viii, 440. 30s. 1947. (Cambridge University Press)

This first volume is subdivided into " Book I ", which is on pure algebra, and " Book II ", on n-dimensional projective geometry. Book I opens with a general account of groups, rings and fields. It includes a discussion of integral domains and of polynomial rings. The term field includes non-commutative fields, or division rings as they are often called.

Chapter II is on linear dependence. It gives a good account of linear sets over fields, which may be non-commutative. The rank of a matrix is defined in terms of the linear dependence of its rows and columns. This definition is shown to be equivalent to the one in terms of determinants, in case the field to which the elements belong is commutative. The chapter closes with an account of the invariant factors and elementary divisors of λ-matrices.

Chapter III deals with algebraic extensions of a commutative field, K, and with algebraic function fields. Algebraic dependence is discussed and the "exchange theorem " is proved for algebraic bases. The treatment brings out the analogy between algebraic dependence in an algebraic extension of K and linear dependence in a vector space over K. Differentiation is introduced, in purely algebraic terms, and is used in finding conditions for algebraic independence. The main theorems on symmetric functions are proved.

The fourth and final chapter in Book I deals with algebraic equations and polynomial ideals. It contains Hilbert's basis theorem, Hilbert's zero theorem and an account of the theory of elimination.

Apart from its applications to geometry, this first part contains a first-class introduction to algebra, in what may be called the van der Waerden tradition.

Book II opens with an algebraic definition of projective space. This starts with a definition of n-dimensional projective number space, $PN_n(K)$, over a field K. A point in $PN_n(K)$ is an equivalence class of ordered sets of $n+1$ elements in K, not all of which are zero. The field K may be non-commutative, in which case one must distinguish between the " right-hand " and the " left-hand " $PN_n(K)$. In the former, two such $(n+1)$-tuples, $(\alpha_0, \ldots, \alpha_n)$ and $(\beta_0, \ldots, \beta_n)$, are equivalent if, and only if,

$$\alpha_i = \beta_i \lambda \quad (i = 0, \ldots, n),$$

for some element, λ, in K.

The points in a (right-hand) n-dimensional projective space, $P_n(K)$, over K are undefined elements which are in a (1–1) correspondence with the points in $PN_n(K)$. This correspondence, and any which is obtained from it by a projective transformation in $PN_n(K)$, is called an allowable coordinate system. The propositions of incidence are proved analytically, and Desargues' Theorem is deduced from them and the observation that any $P_2(K)$ can be imbedded in a $P_3(K)$. Pappus' Theorem is shown to be equivalent to the commutativity of K.

In the next chapter n-dimensional projective space, S_n, is re-defined, starting with the axioms of incidence, together with Desargues' Theorem as a separate axiom if $n = 2$. The theory of harmonic conjugates is developed, without the assumption that the diagonal points of a quadrangle are collinear. This means that every point on a line, l, may coincide with its harmonic conjugate with respect to every pair of distinct points on l. It is shown, by methods similar to those of Veblen and Young, how coordinates, in a field K, may be introduced, so that S_n is a $P_n(K)$. This is done by means of a series of rather complicated geometrical constructions, which could, perhaps, have been

avoided.* However, these two chapters give what is, so far as I know, the most compact account of the foundations of projective geometry, including the logical equivalence of the algebraic and synthetic definitions and, in particular, of the fact that Pappus' Theorem is equivalent to multiplication being commutative.

The next chapter gives an account of Grassmann coordinates, the field K being now commutative and without characteristic (*i.e.*, $1+1+\ldots+1 \neq 0$). It is proved that $\binom{n+1}{k+1}$ given elements in K are Grassmann coordinates of a linear k-space in S_n if, and only if, they satisfy the quadratic relations, which generalise the familiar relation between line coordinates in S_3.

The two final chapters are concerned with collineations and correlations, which are discussed in terms of their elementary divisors. Normal forms are given for collineations, polarities, null-polarities and for general correlations. Comments on the choice of the rather specialised subject-matter of these last three chapters should clearly wait on the appearance of the second volume.

J. H. C. W.

Projective and Analytical Geometry. By J. A. Todd. Pp. x, 289. 25s. 1947. (Pitman)

This volume is designed for use by students in the last two years of a University Honours Course and is based upon lectures which the author has given to undergraduates at Cambridge in preparation for Part II of the Mathematical Tripos. The treatment is strictly algebraic throughout, more so indeed than the title suggests ; but this matter is made quite plain in the excellent introductory pages. Throughout the work the technique of matrices is freely used. The book fulfils a want and is likely to be very useful.

Chapter I deals with Linear Spaces and Coordinate Systems of any number of dimensions. The matrix of $n+1$ rows and one or more columns, whose components are complex numbers, is taken to be fundamental, that of one column being called a vector. Those vectors which are identical save for a constant non-zero factor throughout their components constitute a *ray*. Linear dependence or independence among the rays leads to the concepts of a projective vector space, and the dimension of a space and of its subspaces. Points are defined as the members of a class of entities which are in (1, 1) correspondence with these rays. This leads to coordinate systems, intersections, joins, properties of incidence, principle of duality and projectivities. A notable feature is the neat handling of the line coordinates in three dimensions. Chapter II deals in detail with projectivities, ranges and pencils, introducing the projective invariants of binary forms and covering the fundamental properties of involution and harmonic ranges. Chapter III discusses the conic, defined by the intersecting related rays of two pencils, and leads in a quick and orderly development through the properties of polarity and involution to a geometrical interpretation of the binary cubic, and to Pascal's Theorem and its extensions by Steiner, reaching eventually to the (m, n) correspondence and the poristic properties involved in the symmetrical (2, 2) correspondence. Chapter IV continues in the same vein with quadric surfaces, twisted cubic curves and the linear complex and Klein's treatment of lines in [3] as points upon a quadric primal in [5]. Chapter V develops the theory of canonical forms by rational transformations ; it is in fact an account of the invariant factors and elementary divisors of a matrix, developed with a pleasantly geometrical flavour. This is followed by a short account of the classical canonical form, and then by some illustrations of elementary divisors. Chapter VI deals with

* See E. Artin, "Coordinates in Affine Geometry", *Reports of a Mathematical Colloquium*, University Press, Notre Dame, Indiana (1940).

linear systems of conics and quadrics, and particularly with pencils. This leads to the discussion of the canonical form and all the distinct types of pencil corresponding to the different sets of elementary divisors. Chapter VII elaborates the invariants and covariants of two conics and also of two quadric surfaces. This chapter is a mine of information and useful results which are conveniently assembled together. It contains an account of combinants, a branch of the theory to which the author has recently made notable contributions. A certain unity is achieved by the systematic use of matrices and particularly of the traces of matrix products. Thus if **a**, **b**, **c** are the symmetric matrices, denoting in point form the two conics and their harmonic contravariant conic respectively, and if **A**, **B**, **D** are the matrices (adjoint in structure) for the tangential forms of the conics and their harmonic covariant conic, then, for example, the trace of the product **Cd** is that of the product **aCbC**, and is in fact $\Theta\Theta' + 3\Delta\Delta'$. The chapter naturally contains a geometrical interpretation of the binary quartic in terms of a pencil of quadric surfaces; also it deals with apolarity and self-polar configurations.

There is a great consistency of treatment throughout the book, and the author has done what he sets out to do. He has also taken the risk of writing something which the geometer might criticise as sheer algebra and which the algebraist might criticise as unnecessarily geometrical. But in the present state of highly specialised knowledge this is a risk which should be taken, especially when the author is one who has talent and experience within both regions of the subject. Only, in such an undertaking it is well to bear in mind the two types of reader, the enthusiast for geometry who is to be interested in algebra and the other enthusiast who must be wooed towards geometry. Therefore plenty of references should be given to tempt the reader outwards along new paths. This is indeed done in many places of the present work but it could with advantage go further. Where, for example, is the geometer to find the source of the intriguing theorem of p. 26 that reduces the (1, 1) correspondence which is *free of exceptional elements* to the form

$$(axx^* + bx + cx^* + d)^m = 0,$$

with m a positive integer?

The history of projective geometry is briefly and clearly given in the introduction where two main sources are recognised, the fundamental and synthetic source of von Staudt and the subsequent analytic source, due to Klein. It is this latter which furnishes the technique and the motif of the present work to be set over against, and as a tribute from a pupil to, the pioneer work of Baker in this country upon synthetic geometry. To the present reviewer there seem to be not two but three main sources, that of Grassmann being equally significant with that of Klein in the analytic field. Since this third approach is in close touch with the pure geometry, as also with the use of coordinate systems and the invariant theory, some reference to it and to its later developments by Whitehead, Study and others would have been welcome.

It is a measure of the value of this book that it stimulates thought, especially when one sees familiar facts in new settings. Occasionally one would like to see an interesting feature amplified. When, for example, the author passes from the rational, canonical form of a collineation to the classical form " as somewhat simpler " (p. 165) he hardly does justice to his theme as he has already constructed it. The beauty of the preceding algebra consists in the harmony between its rational processes and the geometrical properties of incidence; the simplicity now to be introduced consists in extracting the roots of an equation of order r. But is this useful process of splitting the nucleus to be admitted into the fine region of projective geometry as a matter of course? Perhaps the algebraist is unduly sensitive here just as a geometer

might be if an algebraist were to talk summarily of the projective distance between two points!

There are details of notation that call for remark, one being the use made of the symbols u and x for vectors. Confusion may easily be caused for readers who consult those books on matrices which are advocated by the author, in that he writes $u'x$ where one of the cited books consistently writes ux. That is, u is defined as a column of elements and then is transposed and called u'. Surely it is simpler to admit outright that a rectangular matrix can be regarded as so many rows and so many columns, each row constituting a row-vector and each column a column-vector. Also the triple reference marks and the asterisks which abound throughout the book are distracting; otherwise the appearance of the page (despite the closely packed printing of a postwar book) is pleasing. The dignity that has come to projective geometry through the disciplined attention, such as this book gives, to the simple essentials undistracted by thoughts on parallels and distance deserves a visible expression of notation and language that reflects this inherent beauty. Of this beauty, there is plenty in this little volume which will bring enjoyment and profit to many another reader. H. W. T.

Éléments de Mathématiques. By N. BOURBAKI. Première Partie: Les Structures Fondamentales de l'Analyse.

I. Livre I, Théorie des Ensembles: Fascicule de résultats; 50 pp. (Actualités scientifiques et industrielles, 846)

II. Livre III, Topologie générale: Ch. 1, Structures topologiques; Ch. 2, Structures uniformes; 129 pp. (Actualités, 858)

III. Livre III: Ch. 3, Groupes topologiques (théorie élémentaire); Ch. 4, Nombres réels; 158 pp. (Actualités, 916)

IV. Livre II, Algèbre: Ch. 1, Structures algébriques; 165 pp. (Actualités, 934). (Hermann, Paris)

To our grandparents, Bourbaki was a gallant and adventurous general of the Franco-Prussian War. The name is likely to become more familiar to mathematicians of the present generation as that of the titular author of the publication whose first four numbers are listed above. These four numbers are merely a beginning: from the indications they contain it is clear that many more are to be expected and the complete work should cover a considerable amount of shelf-space. But exact details of the plan as a whole are lacking.

However, the portions now to hand, with their accompanying explanations, suffice to give a rough idea of the nature of the work. Perhaps it may best be described as a treatise on the whole of pure mathematics as it exists to-day, integrated by an emphasis on the more fundamental abstract ideas and their logical interrelations, but without any encyclopaedic aims in the treatment of detail or special developments. It plans to show modern mathematics (so far as may be) as a unified body of abstract thought; to make clear the mutual dependence of its various branches and subdepartments by allowing them to unfold as it were by the germination of a few simple, seminal ideas; and, generally speaking, to provide a guide to the most frequented paths through the mathematical jungle. Most mathematicians will probably agree that these aims, if I have interpreted them correctly, are entirely worthy of applause. I think also that the undertaking is timely. The comprehensive and general character of the development, particularly of modern algebra and topology, make the task of planning such a work to-day more feasible perhaps than it would have been forty years ago. In these two wide fields a genuine unification has been accomplished in the course of the present century. And it is not

an accident that it is with the foundations of algebra and topology that these first four numbers of *Bourbaki* are concerned.

Bourbaki has none but the most superficial resemblance to such works as the German *Enzyklopädie* or Pascal's *Repertorium*. It is much more like an ordinary mathematical textbook, though on a vast scale ; but it differs from the usual *Cours d'Analyse*, for example, not only in its wider scope, but also in being less of an anthology, in having an elaborate and carefully thought out plan of organisation. Although it is a work of collaboration, no names are mentioned for the authorship of the separate chapters. And the homogeneity of style and treatment in the published portion suggests that there has been on this occasion a much more intimate collaboration than one usually finds in collective work.

Some indications of the formal organisation of *Bourbaki* may be useful. It is to be divided into " parts ". All the numbers so far issued belong to the first part, which is called " Les structures fondamentales de l'analyse ". This first part is divided into " books " and each of these is to be devoted to one of these fundamental structures. The titles of six such books are announced ; they are : Théorie des ensembles, Algèbre, Topologie générale, Intégration, Topologie combinatoire and Différentielles et intégrales de différentielles. Only the first three of these are represented in the four numbers under review. Thus, we have four chapters of the book on general topology, one chapter on the algebra and, of the theory of sets, so far only a " fascicule de résultats ". The latter is stated to be a summary without proofs of those definitions and results of set-theory which are necessary for the understanding of the rest of the work. It adopts throughout the usual naïve point of view, but the authors make it clear that in the main body of this book they intend to take the formalist standpoint. But it is not obvious whether this first book will amount to a regular treatise on mathematical logic and foundational problems. It is, I think, rather regrettable that Bourbaki has not from the first divulged his whole scheme, at least in outline. As it is, we are at many points reduced to guesswork if we wish to grasp the plan entire. We are, however, allowed to infer that many, if not all, of the subsequent books will be supplied with a " fascicule de résultats " for the benefit (among others) of *lecteurs pressés*. This should prove a popular and useful feature.

It may be desirable to mention at this point such other information as can be gleaned about the remaining contents of the two books whose first chapters are now published. The algebra is to include also chapters on the following topics : linear algebra ; multilinear algebra ; rings of polynomials ; divisibility, ordered groups and elementary divisors ; commutative fields ; non-commutative fields, simple algebras, etc. ; quadratic and Hermitian forms ; and finally the elementary geometries (affine, projective, Euclidean, etc.). The general topology is to include, besides the four chapters listed above, a treatment of the following : the additive group of real numbers and related topological groups, vector spaces of a finite number of dimensions and projective spaces, complex numbers, exponentials, logarithms and trigonometric functions, metric spaces, and also a chapter on " sets of continuous mappings of a topological space into a uniform space ". This is as far as present indications go. In any case, these items should give some idea of the character of these two books, which are only a portion of the first of the parts into which, as already explained, the whole work is to be divided, viz. the part on " fundamental structures ".

As to what the remaining parts will include, we are in the dark, except for the statement that they are to deal with theories " où interviennent simultanément diverses structures ". It is only fair to add that *Bourbaki* uses the word " structure " in a perfectly precise sense, explained at the end of the

fascicule on set-theory. Groups, rings, and modules, as well as the various kinds of spaces—topological, uniform, metric, etc.—are to be regarded as structures in this sense. This idea has been vaguely familiar no doubt for many years. It is welcome to have an exact formulation. It provides a useful common ground of reference for concepts usually considered quite diverse. (Lattices, which are called structures by some authors, are also structures in the quite different and more general Bourbaki sense.)

As regards style and treatment, the authors state that their work will be logically self-contained and will pre-suppose no knowledge of particular results. They set out from the most rudimentary and obvious notions about sets, equivalence relations, functions (in the most general sense) and the like, and propose to develop their plans by pure deduction from this basis. It is admitted that a certain familiarity with abstract mathematical reasoning is a desirable qualification on the part of the reader. And the level of abstraction reached in some places is certainly rather high. But the difficulties of abstract thinking have, I believe, usually been exaggerated. Abstraction makes not only for generality but also for a (possibly rather austere) simplicity and the avoidance of irrelevant complications. And I think that in the main a genuinely elementary style has been achieved. Moreover, the student's task has been lightened in several ways. A well-pondered terminology is maintained consistently throughout. Numerous cross-references mitigate the rigidity of the general framework. Useful glossaries are provided in an end-folder to most of the volumes. Ominous warning marks draw attention to likely fallacies. Exercises of varying difficulty are provided on a much more liberal scale than is usual in continental works. These features among others should add considerably to the didactic value of the book.

Little more need be said in particular about the first number listed above. It explains the usual notions and constructions connected with sets, equivalence, partial order and related matters. The few novelties are terminological.

The second and third numbers comprise the first four chapters of the book on general topology. In the first chapter, based on the notion of a topological space, we find developed the essential properties of open and closed sets, neighbourhoods, and continuous mappings. The most interesting feature is the use made of *filters* (in the sense of H. Cartan). This is roughly equivalent in effect to the use of Moore-Smith convergence. It is important because it allows one to extend the usual results about limits and convergence to the most general and " infinite " kinds of topological space. The chapter ends with a good account of compact (the older bicompact) and locally compact spaces and of connectedness.

The second chapter deals with uniform spaces, or—more exactly—spaces with a uniform structure. This is a kind of structure quite distinct from that of a topological space. But from a uniform structure a topological structure is uniquely determined. And the spaces whose topologies are derivable in this way from a uniform structure are decidedly those of the greatest interest: they include compact spaces, metric spaces and topological groups. The theory of uniform spaces is therefore of central importance. The essential theorems are that every uniform space can be " completed " (the metric case will be the most familiar instance of this), that the topology of a compact space is always derivable from a uniquely determined uniform structure and that the latter is complete, and finally that every continuous mapping of a compact space into a uniform space is uniformly continuous. The third chapter deals with the elementary properties of topological groups, rings and fields and in particular with their " uniformity " and completion. Matters of a more algebraical character, such as the duality theory of locally-compact Abelian groups and representation theory, are doubtless reserved for a later

chapter. Finally, in chapter IV, on the real numbers, contact is made for the first time with undergraduate mathematics. We find a careful account of the relations between the various structures—topological, uniform, order and algebraical—of which the real numbers are susceptible, the properties of continuous and semi-continuous numerical functions of one variable, infinite series and products, etc. Thus we are led from the abstractions of the twentieth century to the classical results of the nineteenth.

The fourth number forms the first chapter of the book on algebra. It is based on a definition of an algebraic structure which is broad enough to accommodate as special cases the most important standard types—groups and rings (with or without operators), fields, etc. Nevertheless, I feel that here an opportunity has perhaps been missed. The distinction, in terms of which the definition is framed, between internal laws of composition (for example, the addition of vectors) and external laws (such as multiplication of vectors by scalars) does not appear to be really fundamental. And I believe that a treatment which avoids this rather tiresome distinction by admitting only "external" *operators*, but allows these to have any number of arguments (not merely one or two)—such as is to be found, for instance, in the papers of G. Birkhoff and A. Tarski—would have been more attractive aesthetically, besides being more general. However, the greater part of the chapter is concerned with the standard types of algebraic structure mentioned above and deals with such matters as permutation groups, the theorems of Schreier and Jordan-Hölder for groups with operators, simple properties of ideals, etc., etc. The numerous exercises are most valuable in providing a ballast of illustrative detail.

Only the most receptive minds can hope nowadays to acquire a thorough knowledge of more than a few branches of mathematics. *Bourbaki* promises the opportunity of a bird's-eye view of the essentials of at least all the more abstract and well-connected portions of the subject. One of its main merits is, in fact, the perspective, the grading of emphasis and subordination of detail, which alone make this possible in a reasonable compass. Whether the compass ultimately required will in fact prove to be reasonable remains, of course, to be seen. But, however that may be, it remains a unique enterprise which deserves a warm welcome from all who wish to follow the unfolding of modern mathematical ideas. P. H.

Algèbre et analyse Linéaires. Par A. LICHNEROWICZ. Pp. 316. 800 fr. 1947. (Masson, Paris)

In the space at the reviewer's disposal it is impossible to do more than hint at the great wealth of material to be found in this book. It is divided into two parts, the first of which is entitled "Linear Algebra" and the second "Linear Analysis".

In Part 1, the reader is introduced to vector spaces, change of basis and rank. This leads to a discussion of linear equations, consistency and normalised orthogonal bases. Determinants and their properties are then treated from a somewhat abstract point of view. Next come matrices and the solution of simultaneous equations.

Chapter II is a good one dealing with Euclidean and Hermitian space, Schwarz's inequality, the orthogonalising process of Schmidt and the Bessel-Parseval inequality, all this being in preparation for Chapters II and III of Part 2.

Chap. III returns to matrix theory. Matrix rings, functions of matrices and the collineatory group of transformations follow. Next comes a section on symmetric and Hermitian matrices with their associated quadratic forms, latent roots and Neumann's series. Lastly, the Jordan canonical form of a

matrix with distinct latent roots, and the reduction of Hermitian matrices are established.

Chap. IV deals with tensor algebra and includes change of basis, contraction, outer products and polars.

Part 2 extends the underlying ideas of Part 1 to analysis and begins with a chapter on the tensor calculus and differential forms. This leads to multiple integrals and Stokes's theorem for n dimensions. The theorem of Gauss (or Green) appears under the unfamiliar name of the " formula of Ostrogradski ".

Chap. II begins by introducing the concept of a vector space of generally continuous functions and its generalisation to Hilbert space. Systems of orthonormal functions, convergence in the mean, and complete systems, are followed by Bessel's inequality and the Fischer-Riesz theorem. An investigation of the properties of orthogonal polynomials then brings one to Fourier integrals and Fourier series.

Chap. III deals with linear operators, in particular Hermitian operators and the analytical representation of bounded operators in Hilbert space. This serves as an introduction to infinite matrices and the spectrum of a functional operator.

The last chapter (IV) deals with the Fredholm integral equation of the second kind

$$\phi(s) - \lambda \int_a^b k(s, t)\, \phi(t)\, dt = f(s), \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1)$$

which follows naturally from the linear integral operators of Chap. III. Beginning with the simpler case in which the kernel k is degenerate and in which the solution of (1) can be effected by solving a system of simultaneous linear equations, the properties of iterated kernels and of the kernel resolvent are studied, and Fredholm's theorem exhibiting the most general solution of (1) is established. The remainder of this chapter expounds the properties of symmetric kernels, $k(s, t) = k(t, s)$, and culminates in the Hilbert-Schmidt theorem that every function f which can be written in the form

$$f(s) = \int_a^b k(s,t)\, h(t)\, dt$$

is a linear combination of the eigenfunctions of the kernel.

According to the foreword, the author's intention is to write a book for French physicists comparable in scope with the classical works of Courant and Hilbert or of Frank and von Mises, but differing from these works in two points. On the one hand a more elementary treatment is aimed at and on the other, the emphasis throughout is laid on the algebra of linear operators. The author also states that he has found it impossible to renounce his standards of rigour, without which, he says, the book would be neither Mathematics nor Physics. At this last statement few Pure Mathematicians would cavil but it is difficult to imagine the average British physicist submitting to the rigour and abstraction of this book. Much of it will delight the Pure Mathematician but in other sections, particularly in Part 1, he will be discouraged by awkward and sometimes unfamiliar notations. The author's determination to relate every possible topic to the nomenclature of vector spaces does not always make for lucidity. This is particularly evident in the sections dealing with determinants and matrices. Thus a determinant is defined as a scalar function D of the n vectors $\vec{A}_1, \ldots, \vec{A}_n$ of a vector space which satisfies the following postulates

$$(a)\ D(\vec{A}_1, \ldots, \vec{A}_i, \ldots, \vec{A}_j, \ldots, \vec{A}_n) = -D(\vec{A}_1, \ldots, \vec{A}_j, \ldots, \vec{A}_i, \ldots, \vec{A}_n),$$

$$(b)\ D(\vec{B}_1 + \vec{C}_1, \vec{A}_2, \ldots, \vec{A}_n) = D(\vec{B}_1, \vec{A}_2, \ldots, \vec{A}_n) + D(\vec{C}_1, \vec{A}_2, \ldots, \vec{A}_n),$$

(c) $D(\alpha\vec{A}_1, \vec{A}_2, \ldots, \vec{A}_n) = \alpha D(\vec{A}_1, \vec{A}_2, \ldots, \vec{A}_n)$,

(d) $D(\vec{e}_1, \ldots, \vec{e}_n) = 1$,

where $\vec{e}_1, \ldots, \vec{e}_n$ form an orthonormal basis. Undoubtedly the last three chapters in which vector spaces are least evident, are the best.

The abstract nature of the book is emphasised by the fact that worked examples are almost completely absent. The work contains an index, which might have been considerably extended with great advantage to the reader. The printing, too, is better than in most French books, but there is a large number of printers' errors, particularly in regard to suffixes and accents.

In spite of the above criticisms, this work has much to recommend it, for it does present a coherent and logical account of the linear operators used in modern physics. The reviewer thoroughly enjoyed reading this work of Professor Lichnerowicz but he would hesitate before recommending it to his colleagues in Physics. D. E. R.

Lewis Carroll. By F. B. LENNON. Pp. 358. 15s. 1947. (Cassell)

No really satisfactory study of Lewis Carroll exists. This assertion may seem unkind to Mrs. Lennon, who has toiled with patience and enthusiasm to produce the present volume. To a fanatical Carrollian, any book about Carroll is almost certain to be better than none, and Mrs. Lennon's book must be added to the sacred shelf; but to those who do not know their Carroll by heart, who have not eagerly devoured every available scrap of knowledge concerning him—it is, I suppose, possible that such ignorant and benighted souls exist—to these, we can hardly recommend it as an explanation of why "Dodgson, lecturer on geometry at Oxford, ... gave birth to one of the most famous stories of all time".

It is not enough, at least in the *Gazette*, to say this without amplification. Lest such amplification may seem to be ungracious to an author who has striven hard to please us, let me emphasise that her work is not sketchy, nor slovenly, her style, though at times florid, is not displeasing, and her attitude to Carroll is in the main neither patronising nor Boswellian.

To show the main figure in appropriate setting, a setting probably quaint and unfamiliar to American readers, Mrs. Lennon has devoted great industry to portraying Victorian England, and, in particular, Victorian Oxford. A faint flavour of Lytton Strachey's artifices clings to her account, unnecessarily, for Victorian Englishmen were no more—if no less—absurd and grotesque than any other set of human beings of which history bears record. Further, a fully pictured background must be an ordered aggregate of details, and if we find in these details numerous errors, trifling and insignificant in themselves though they be, we are led to doubt the accuracy of the picture as a whole. For instance, it is a trivial error to refer *The Vulture and the Husbandman* to an Oxford magazine, but the implied ignorance of A. C. Hilton diminishes our confidence in the author's judgments on English wits and parodists; it is a trivial if blatant error to say that H. J. S. Smith was the only contemporary lecturer on modern geometry in any British university, but it weakens our faith in the author's opinions on mathematical topics.

The main figure is drawn with sympathy; I cannot say, with understanding. Is Lewis Carroll too simple a character to evoke a good biography? Is there any need to look for subtleties which do not exist, save in the esoteric fancies of our wilder psychologists? There is a belief that Carroll wrote nonsense books *in spite* of being a mathematician, that he was a Jekyll and Hyde of the world of thought. This is not merely crude; it cannot resist for one moment

the stroke of Occam's razor : there is no need to postulate duality for a man who, whatever his oddities, was a completely integrated personality. Mrs. Lennon does not quite swallow the dual personality, but she hankers after it, and hardly realises that Carroll wrote the Alice books *because* he was a keen mathematician, though not a good one. The essential ingredients of Wonderland are logic and imagination, precisely the qualities of the creative mathematician. And if we wish to estimate aright the quality of Dodgson's mathematical talent, we must seek for it in the right place—not in his *Condensation of Determinants* nor in his *Syllabus of Plane Algebraic Geometry*, but in Wonderland and through the Looking-glass. Grant that Carroll was odd ; grant that his passion for small girls bordered on the perverse ; grant that, like any other human being, he was a complex jumble of a dozen straining threads ; there is yet no need, granted all this, to picture him " slipping back and forth " by " a bridge firmly rooted in both sides of his personality ". The mathematician rejects this dichotomy ; the poet, in the person of Mr. Walter de la Mare, will have none of it ; so let it be forgotten. Instead, let us remember with delight the many happy incidents of which Mrs. Lennon reminds or informs us : Carroll making Macmillans buy theatre tickets for him ; Carroll skirmishing with Tenniel and Furniss ; Carroll fussing with his gadgets ; Carroll staying small damsels, not with flagons but with safety-pins. And above all let us thank the author for giving us an excuse for yet another reading, another escape to a world of which the Red Queen might have said " compared with the real world, it's as sensible as a dictionary ". T. A. A. B.

Mathematical Theory of Rocket Flight. By J. B. Rosser, R. R. Newton and G. L. Gross. Pp. viii, 276. 22s. 6d. 1947. (McGraw-Hill)

The authors of this book were engaged, during the war, on rocket research and development at the Allegany Ballistics Laboratory. This work is their official report to the United States Offices of Scientific Research and Development and is concerned with the mathematical exposition of external ballistics of fin stabilised rockets. It is the first published account, in any serious detail, of the fundamental principles of rocket flight as security reasons have hitherto prevented open publication of a theory that has been in existence for many years. Although the report covers work started as late as 1944 it deals with the then existing knowledge and pays gracious acknowledgment to earlier workers in the United States and particularly to those in the United Kingdom who, headed by Mr. W. J. Cook of the Research Department, Woolwich, laid the foundations of rocket ballistics in 1936.

Anyone who has had any experience in formulating the basic rocket equations will appreciate the need for extreme care in so doing, in view of the rapidly changing mass of the body whose motion he wishes to establish. The authors rightly draw attention to the difficulties in their initial paragraphs and go to some trouble clearly to obtain in a rigorous manner the fundamental equations. They enunciate a " special principle " in an attempt to overcome the difficulties but this, on examination, does no more than emphasise the necessity for the careful application of Newton's laws to a clearly defined system of particles. Further principles or what might better be called theorems are enunciated and proved and the equation of motion established. This treatment, which is restricted to rockets unrotated or having only a slow spin, *i.e.* not gyroscopically stabilised, forms a most instructive first chapter.

The motion of a rocket has three phases in its complete trajectory ; the initial launching period when it is burning and constrained by its projector, the remainder of the burning period in free flight and the subsequent period after burning has ceased. It is not difficult to determine the ideal trajectory

composed of these three parts although considerable arithmetic may be necessary. It is the deviation of a single rocket, giving rise to the dispersion of a group of similar rockets, from the ideal path that is the difficult but nevertheless interesting problem for the ballistician. It has not yet been possible to relate the observed dispersion or deviation of a rocket to the theoretical value calculated from assessed values of the physical quantities which are believed to be responsible for the dispersion. The calculations involved are by no means simple and the detailed review of the methods, so far used, given in chapters two, three and four should provide a sound basis for the future ballistician who would attempt to solve this well-known rocket problem. It is the third chapter which contains the most important part of this work, being devoted to the theory of the motion during the free-burning period. Numerical examples from active rocket firings are given and a very full discussion presented. Chapter two provides the necessary equations governing the period after burning has stopped ; while chapter four deals with the initial launching phase which provides the boundary conditions for the main problems.

It was a pleasant surprise to find chapter five " Properties of Rocket Functions ". Here is gathered together a considerable amount of information on the integrals that are required in the deviation theory ; but much more than is necessary and the collection of evaluated integrals will be of value to others than those primarily interested in rocket theory. Tables of the essential functions occupy seventeen pages.

The book is most pleasing to read and very well produced. It has been written for two types of reader : the rocket ballistician and, to quote from the dust cover, " the reader with little scientific training, who is interested in what makes the rocket go ". The book answers such questions as " why does a rocket fired into a crosswind turn upwind? " R. C. K.

Practical Analytic Geometry with Application to Aircraft. By R. A. Liming. Pp. xvi, 277, tables and index. 25s. 1944. (Macmillan)

This book describes the application of coordinate and projective geometry to the discussion of aircraft structures and shapes. From the point of view of the teacher of mathematics it may well provide examples of practical applications of a subject whose interest is largely intrinsic.

The book is divided into three parts. The scheme followed in the first two parts is to quote a standard formula or equation without proof, and to follow this with one or more numerical examples and then by an aircraft application. The earlier chapters are concerned with the straight line (in the two and three dimensional cases) and the plane. Among the ideas that find application are direction cosines, rotation of axes and the distances and angles between lines and planes.

The third part is devoted to a process described as " conic lofting ". This consists of fitting portions of conics to the various contours of aircraft surfaces. The condition of streamlining necessitates that the surfaces, not only of the lifting and control systems, but also of the fuselage, engine nacelles, cockpits and other protrusions should be curved. The precise form of the curves is empirical, but the practical problems of design and production make it highly desirable that some definite type of curve should be used. This enables algebraic methods to be applied and numerical data to be given to any desired degree of accuracy.

Curves of the second degree are particularly convenient for this purpose, and their application is discussed in detail with numerous examples. These are reduced where possible to the derivation of the conic which touches two given lines at given points and passes through a third point. The equation is

obtained, and in addition a geometrical method of plotting based on Pascal's theorem is given.

Suggestions for setting out the numerical work in tabular form are given and the book liberally supplied with carefully drawn diagrams. It is a little strange to see numerical examples worked to eight decimal places, but the author assures us that this is common practice. An appendix contains eight-figure tables of the six circular functions. B. M. B.

Analytic Geometry. By D. S. NATHAN and O. HELMER. Pp. 402. $3.75. 1947. (Prentice-Hall, New York)

This is a well-written and well-produced textbook, genuinely on analytical geometry and not merely on conic sections. It begins with directed line-segments, coordinates on a line and rectangular coordinates in a plane, directed angles, and loci. There is a good emphasis on equations of loci and " loci of equations ". There are then short chapters on lines, circles, and transformation of coordinates. The conics are taken first in standard and then in arbitrary position. There are chapters on higher plane curves, and on parametric representation and polar coordinates. The two-dimensional portion ends with an interesting account of the fitting of empirical data.

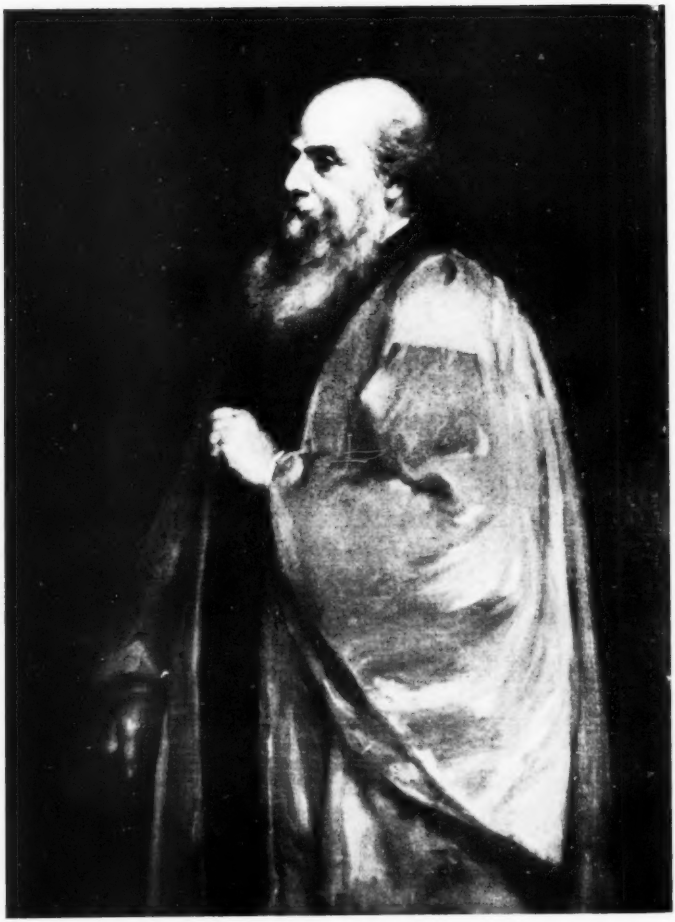
There is a curious neglect of tangents. In the index are two references to " tangent line " : one is to a worked example about a circle and the other to a question set to the student on the parabola, but it is true that in this question a hint is given how to single out the member of the family of nonvertical lines through the point (x_0, y_0) of $x^2 = 2py$ which has no other point in common with the parabola. It follows that there is no consideration of envelopes or duality. The explanation is evidently that the authors are unwilling to use calculus. Parameters are introduced too late to be of much use.

In about 80 pages is given a good introduction to the use of coordinates in space. Besides the plane, line, and direction cosine this includes some examples of (surface) loci, with good diagrams and examples of space curves.

There are about 1500 questions for the student, with answers to odd-numbered ones. There is good variety in these questions, but they are scarcely " problems " in the older sense of the word. Some of them are not closely attached to the subject-matter of the chapters in which they occur, *e.g.* there is a question about the sales of radio sets in the chapter on conic sections in arbitrary position which only amounts to finding x so that $(30-x)(20+x)$ may be greatest. A. R.

1581. (895). In his wretched life of less than twenty-seven years Abel accomplished so much of the highest order that one of the leading mathematicians of the nineteenth century (Hermite, 1822–1901) could say without exaggeration, " Abel has left mathematicians enough to keep them busy for five hundred years ". Asked how he had done all this in the six or seven years of his working life, Abel replied, " By studying the masters, not the pupils ".—E. T. Bell, *The Queen of the Sciences* (1931), p. 10.

PRINTED IN GREAT BRITAIN BY ROBERT MACLEHOSE AND CO. LTD.
THE UNIVERSITY PRESS, GLASGOW



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